THURSDAY, AUGUST 22, 1895.

TWO BOOKS ON ARCTIC TRAVEL.

The Great Frozen Land. By Frederick George Jackson. (London: Macmillan and Co., 1895.)

Ice-bound on Kolguev. By Aubyn Trevor-Battye. (London: Archibald Constable and Co., 1895.)

BOTH these books are well worthy the attention of every one interested in Arctic travel. But little was known about the island of Waigatz, and still less of Kolguef. Both books are profusely illustrated, and provided with useful maps, but some of Mr. Jackson's pictures are borrowed without acknowledgment. As might naturally be expected, the Samoyedes occupy the greatest share of attention, but some information respecting the fauna and flora of both islands is added, and the difficulties of travelling are dwelt upon with considerable detail.

The "Great Frozen Land" has been compiled by Mr. Arthur Montefiori from Mr. Jackson's journal of his trip across the tundras of European Russia, from the Kara Gates to the Varanger Fiord *viâ* Ust Zylma and Archangel. In one of the appendices, Mr. Montefiori explains the object, method, and equipment of the Jackson-Harmsworth Polar Expedition, and in another appendix Mr. Joseph Russell Jeaffreson adds some notes on the ornithological results of Mr. Jackson's journey.

The narrative begins on August 25, 1893, outside the lagoon of the Pechora, and ends on January 18, 1894, at Vadsö, the frontier town of Norway. The greater part of the book has been devoted to the Samoyedes, but the real object of the journey was neither ornithological nor anthropological, otherwise it would not have been undertaken in winter. Mr. Jackson, as everybody knows, was planning an expedition to Franz Josef Land, and the very practical idea occurred to him that a winter among the Samoyedes must give him a personal acquaintance with the difficulties of land travelling in the high north, and might suggest a successful way of battling with some of them.

Mr. Jackson must be congratulated upon his editor. Mr. Montefiori has spared no pains to make the book interesting. The information which Mr. Jackson himself procured, especially on the island of Waigatz, is valuable, and it is supplemented by quotations from Rae, Castrén von Strahlenberg, Purchas his Pilgrimes, and the works of various other travellers.

Unfortunately the ornithological part has not fallen into such good hands. There are a dozen or more gross mistakes in the spelling of the names of the birds, and in addition there are some curious inconsistencies. In the preliminary observations we are told that Mr. Jackson brought home "of swans—not Bewick's—but the common variety of that region," in spite of which the only swan in the list (No. 28) is Bewick's swan. Mention is made of grossbills. (Does the writer mean crossbills or grosbeaks?) Of the little stint (No. 45) it is stated that the only authentic eggs were those taken by Middendorff. There is no reason to believe that Middendorff ever found the eggs of the little stint. The eggs which he records as being those of *Tringa minuta* were probably those of *Tringa ruficollis* and possibly those of *Tringa subminuta.* The first identified eggs of the little stint were taken on July 22, 1875, by Mr. Harvie-Brown, on the eastern shores of the lagoon of the Pechora, and a few days later a score had been obtained by the expedition. Other eggs equally authentic have since been taken in Lapland, Novaia Zemblia, and Kolguef. It is extremely unlikely that the identification of the species in the list is always correct. No. 10 doubtless refers to *Phyllosopus tristis*, and not to the chiffchaff: No. 12 is more likely to be a redpole than a siskin; No. 39 is doubtless *Ægialitis hiaticula*, and not *Æ. curonica*, and No. 53 is more likely to be *Stercorarius richardsoni* than *catarractes*. In but few cases is the exact locality given, so that on the whole we must condemn the list as worse than useless.

Mr. Jackson went out on one of Captain Wiggins' numerous voyages to the Yenesei, and was left on the southern shore of the Yugorski Strait, with little or no knowledge of the language of the country, to fight his way as best he could. He was anxious to go to the Yalmal Peninsula, but the Samoyedes declined to take him there. After reading the account of the difficulties which Drs. Finsch and Brehm encountered, it must be admitted that their decision was very wise. Mr. Jackson was, therefore, obliged to content himself with exploring Waigatz Island, and succeeded in making the detour in a fortnight. The north of the island enjoys a milder climate in winter than the south, the Yugorski Straits being frozen over, whilst there is always more or less open water in the Kara Gates.

Winter came upon the adventurous traveller rather suddenly during the second week of October, and on the 13th he began his sledge journey to the Norwegian frontier. During the three months that this occupied, Mr. Jackson lived among the Samoyedes and picked up many useful hints as to dress, food, &c., as well as accustoming himself to camping out in the snow, travelling by sledge, using snow-shoes, &c. This information and experience will doubtless be of great value to him on his expedition to Franz Josef Land. It is worth something to know, instead of only to suspect, that you have pluck to face the difficulties of Arctic travel, and every one wishes a safe return to a traveller who with but small previous experience has gone to try his luck in battling with enormous difficulties.

Mr. Trevor-Battye's book treats of the journey which he made in 1894 to a still less known part of the Arctic Ocean. The island of Kolguef lies about 150 miles to the west-north-west of the lagoon of the Pechora, whilst the island of Waigatz lies about as far to the north-east of that basin. Mr. Trevor-Battye sailed from Scotland in the steam-yacht *Saxon* on June 2, and landed, with his bird-skinner, on the west coast of Kolguef on the 16th; but as ill-luck would have it, they went again on board, and did not finally leave the vessel until the 21st, after the ice had driven them to the north of the island. On August 18, a Russian merchant from the Pechora arrived on Kolguef, and Mr. Trevor-Battye and his companion left in his boat on September 18, and after a nineteen hours somewhat perilous sail, reached the mainland. In two months he was back again in England.

Mr. Trevor-Battye appears to have kept a copious journal, and very interesting reading it is. It bears internal evidence of having been written on the spot by

one who was well trained in habits of observation, and accustomed to the drudgery of making daily notes of what he saw. The remarks on the peculiarities of the Samoyedes are valuable from their originality, and are an important contribution to the ethnology of Siberia in Europe. The value of the ornithological appendix is in strong contrast to that in Mr. Jackson's book; but it must always be remembered that Mr. Trevor-Battye is h mself an ornithologist, and travelled at a time of year when the country was full of birds. Mr. Jackson makes no pretension to any knowledge of ornithology, he travelled at a season when birds were very scarce, his mind was occupied with other thoughts, and he had the misfortune to entrust the few skins he brought home to hands as inexperienced as his own.

Mr. Trevor-Battye's account of the way in which the Samoyedes surround the geese when most of them are unable to fly, because they are moulting their quills before migrating to the coasts of Western Europe to winter, is most graphic.

On the south-east coast of Kolguef the sea is shallow, and at low tide there is much sand exposed within the line of the outer barrier of piled-up ice, which lies some three miles out to sea. In this lagoon thousands of geese retire towards the end of July to moult their flight feathers. When they are in this more or less helpless state, the Samoyedes slip down in their boats through the fog and get behind them, and gradually drive them on shore, where a decoy net has been staked out to receive them. Once inside this trap they are slaughtered without mercy to provide food for the winter. The day's bag was 3300 brent geese, 13 bean geese, and 12 white-fronted geese. Fortunately for the two species of grey geese, they moult a little later than the black geese, so that most of them were able to fly. The Samoyedes told our travellers that the bernacle goose nested at the north of the island.

Mr. Trevor-Battye was fortunate enough to obtain eggs both of the grey plover and little stint. Mention is made on page 209 of the capture of two examples of the curlew sandpiper, but curiously enough this bird does not appear in the ornithological appendix.

There is an interesting appendix on the flora of Kolguef. The cloudberry, one of the most delicious of fruits, which is found on the highest summits of the Peak of Derbyshire, and on the Craven Mountains in Yorkshire, was in flower by the second week of June, but the fruit did not ripen before August 25.

Both Kolguef and Waigatz have an island climate, very different from that of continental Siberia; and it might be said of both of them, as is frequently said of Lapland, that they have eight months winter, and four months no summer. The frequent rains are no doubt very favourable to the growth of many species of plants, but they sadly interfere with the pleasures of camp-life. When the north wind brings down fogs from the Arctic ice in June, and snow followed by rain in July, varied with thunder in August, and frosts in September, it requires some enthusiasm for birds or flowers to enjoy the fight with the storms. There are, however, some compensations. If there be little sunshine there is no night, and when the north wind blows the plague of mosquitoes is stayed.

HENRY SEEBOHM.

ANOTHER BOOK ON SOCIAL EVOLUTION.

The Evolution of Industry. By Henry Dyer, C.E., M.A., D.Sc., &c. (London: Macmillan and Co., 1895.)

THIS work contains much valuable suggestion, many admirable sentiments, and a selection of choice extracts from the best writers on social philosophy; but it is hardly what one would expect from its title. The idea of evolution is, no doubt, more or less present to the author throughout his work, and some of its main characteristics are referred to and illustrated by the phenomena of industrial progress; but there is a want of system and of logical connection in the treatment of the subject, and an entire absence of the unity of design, forcible reasoning, and original theory which were such prominent features in Mr. Kidd's work.

Dr. Dyer's book is an eclectic one, inasmuch as it adopts from previous writers such ideas and principles as commend themselves to the author. His frequent quotations are often followed by the remark—"there is much truth in this"—and it is sometimes rather difficult to determine what are his own conclusions. It would not be difficult for both individualists and socialists to find support here to their own views; but the general impression made by the volume is, that the author is profoundly dissatisfied with the present state of society, and is inclined to some form of socialism as the only effective remedy.

In the introductory chapter we find many of the objections to socialism very strongly put, though most of these are objections to particular details rather than to essential principles; yet in the same chapter we find statements of fact which answer many of these objections. Thus we are told (p. 21): "Among the co-operators, for instance, we find men managing, with the highest efficiency, concerns of great extent and importance for salaries smaller than those of bank clerks. They find their real salaries in the success of their work, and in the knowledge that it will lead, not simply to individual riches, but to the welfare of the community, and especially of the workers."

After quoting from the late Prof. Cairnes to the effect that no public benefit of any kind arises from the existence of an idle rich class, he adds: "From a scientific point of view, and therefore from a moral point of view, no man or woman, unless physically or mentally disabled, has any right to remain a member of a community unless he or she is labouring in some way or other for the common good. In every organised society, therefore, there can be no rights apart from duties" (p. 37). This principle is thoroughly socialistic, and would lead us very far indeed; but here, as elsewhere, the author seems afraid to carry out his own principles to their logical conclusions. Further on, he tells us that—"In some parts of the country as much as between 40 and 50 per cent. of all the deaths that occur are those of children under five years of age, a state of matters which is a disgrace to our civilisation"; and, after quoting some forcible words of Lady Dilke as to much of England's industrial greatness being due to her practically unlimited supply of the cheap labour of her women and girls, he concludes: "It is therefore evident, both from an economic and a moral point of view, that the individualist system of industry, by itself is not sufficient to bring about a stable social

structure." He describes hospitals as institutions "which are founded for the purpose of taking in some of the waste products of our industrial and social system, and for repairing, as far as possible, the injuries which they have suffered"; and he adds: "Such institutions are sometimes pointed out as the glories of our civilisation. They should, on the contrary, be looked upon chiefly as monuments of neglected duties, and the object of all social reformers should not be to extend them, but so to improve social and industrial conditions as to render them almost entirely unnecessary." This will be a new idea to many good people, but it shows that the author is far ahead of the average social reformer.

Again, he points out that the armies and navies of the world afford most instructive lessons in collective action, and that it would be equally possible to have armies of men organised for industrial work, and navies for carrying on such commerce as was essential for supplying the wants of the community; and in his chapter on "Industrial Training," he shows how necessary it has become to supplement the very imperfect means now afforded to apprentices to learn their business by some systematic and well-organised system under local or other authorities.

In the iast chapter, on "Industrial Integration," suggestions are made as to the course of future legislation. The author thinks that it will be made increasingly difficult for people to live upon unearned incomes, while the equalisation of opportunities will reduce the rewards of extra ability. How this is to be effected is not made clear; but the author is decidedly of opinion that "the resumption of the ownership of the land by the community is a first essential to equality of opportunity"; concluding with the rather weak remark, that "the methods to be adopted to bring this about will require very careful consideration, and must be comparatively slow in their operation."

After quoting the opinion of the late Mr. Werner Siemens, that the progress of science will lead not to the increase of great factories, but to the return to individual labour, Mr. Dyer adds:—

"The factory system will continue, and no doubt be extended, for the supply of the common necessaries of life, but the applications of electricity and other methods of obtaining motive power will enable large numbers of small industries to be carried on in country districts. This movement will ultimately bring about a society of integrated labour, which will alternate the work of the field with that of the workshop and manufactory. In order that the evils arising from unlimited competition may be avoided, these departments of work will all be so coordinated that a considerable region will, to a large extent, be self-contained as regards its requirements, and will produce and consume its own agricultural and manufactured necessaries of life."

This conclusion has been reached by the present writer and some others, mainly from broad considerations of economy. But when it is set forth in a work which professes to trace and discuss "the evolution of industry," we expect to be shown that it is a logical and inevitable result of the evolution that has occurred and is now going on. This is nowhere done, and in this respect the book must be pronounced a failure, although there is much in it with which every friend of progress and every student of social science must heartily agree.

ALFRED R. WALLACE.

MAYAN HIEROGLYPHICS.

A Primer of Mayan Hieroglyphics. By Daniel G. Brinton. Publication of the University of Pennsylvania Series in Philology, Literature, and Archæology, vol. iii. No. 2. (London: Ginn and Co.)

ALL who are interested in American archæology (and especially those who do not read German) must feel greatly indebted to Dr. Brinton for his "Primer of Mayan Hieroglyphics," for in this little book he has brought together the result of work done during the last few years in America, England, and Germany, and his own extensive knowledge of the subject of which he treats gives the highest value to his selections and his comments.

That there has been a distinct advance made all along the line cannot now be doubted, and material for study has not only increased, but has been made more generally available to the student

Dr. Brinton divides the Maya inscriptions into their three elements—mathematical, pictorial, and graphic, and proceeds to review them in that order. He first describes Prof. Förstemann's interesting investigation into the Maya notation for the higher numbers, and then enumerates the various divisions of time in use amongst the Mayas, and points out that the bringing of these irregular numbers into unison with the lunar and stellar years is the difficult task which lies before the investigator.

"We need not search" [in the inscriptions] "for the facts of history, the names of mighty kings, or the dates of conquests. We shall not find them. Chronometry we shall find, but not chronicles; astronomy with astrological aims; rituals, but no records. Pre-Columbian history will not be reconstructed from them. This will be a disappointment to many; but it is the conclusion toward which tend all the soundest investigations of recent years."

Whilst dwelling upon the elaborate and careful researches of what may be called the astronomical school of investigators, Dr. Brinton does not fail to give an instance of how far they differ from their rivals, by quoting the explanation given of a certain series of figures in the "Codex Cortesianus," which, in agreement with Förstemann, he supposes to represent the position of certain celestial bodies before the summer solstice, whilst Prof. Cyrus Thomas says of them, "It may be safely assumed that these figures refer to the Maya process of making bread"! Such differences of opinion would seem to indicate that the study of the inscriptions has not yet emerged from the stage of guess-work, and to a great extent this is undoubtedly the case; but it is satisfactory to mark how the happy guess-work of the last few years, and the criticism it has provoked, has led to a solid foundation of ascertained fact from which a fresh start can now be made.

Under the heading of "Pictorial Elements," Dr. Brinton gives us a list of the Maya gods and their attributes, gathered chiefly from old Spanish records. Regarding some of those deities, he has already published some interesting studies in "American Hero Myths." He then proceeds to discuss the cosmogony of the Mayas, and in the following pages deals with the pictorial representations of the Maya divinities, referring continually to the list published in 1892 by Dr. Schellhas in the *Zeitschrift für Ethnologie.*

Students appear to be now fairly well agreed about the order in which the glyphs are to be read, and on the identification of the signs representing days, months, and some of the other divisions of time; but there still remains for consideration a large number of glyphs to which the most varied and contradictory interpretations have been given.

The most essential qualification for a student of Maya inscriptions is without doubt a thorough knowledge of the Maya language as it is now spoken in Yucatan. Dr. Brinton, who is a distinguished philologist, has doubtless learnt all that imperfect dictionaries and grammars can teach him, and on that account alone would hold a foremost position in the investigation. But the only way to acquire the special knowledge which is now so much needed is a prolonged residence in Yucatan itself, which can be reached in five days from New York; and it would be good news should we hear that Dr. Brinton has used his great influence in persuading some of the well-endowed universities or colleges in America to establish travelling scholarships for the study of native American languages, and had placed the Maya language first on the list.

OUR BOOK SHELF.

Harrow Butterflies and Moths. Vol. i. By J. L. Bonhote, M.B.O.U., and Hon. N. C. Rothschild, F.E.S., F.Z.S. 8vo. Pp. xi. and 95. Plate. (Harrow: Wilbee, 1895.)

AT the present day, natural history receives considerable encouragement at our larger public schools and colleges, many of which now boast a Natural History Society of their own, and publish a journal of their own. The naturalists of Harrow School have struck out a bolder path, and have begun to issue a series of manuals of their local fauna, of which this is the second, the first, by Mr. Barrett-Hamilton, having been devoted to the birds of Harrow.

The volume before us includes the *Macro-Lepidoptera* to the end of the *Noctuæ*, and is illustrated by a useful plate presented by the Hon. Walter Rothschild, representing the antennæ of the three British species of *Ino*, the neuration of *Papilio machaon*, and the egg, larva, and pupa of *Vanessa cardui*. The second volume will include the remainder of the *Macro-Lepidoptera*, and the *Pterophoridæ*. South has been followed for Latin names, and Newman for English names, and the indefinite term "variety" has been very properly abandoned.

The district included comprises, roughly speaking, a radius of about five miles from Harrow Hill, and incorporates the notes of a considerable number of observers, the majority being connected with Harrow School. It consists mainly of a record of localities, times of appearance, and habits, with occasional notes on species not found in the district, or on aberrations.

As a record of the present fauna of a restricted locality, this little book will be of permanent value, in view of the changes which are always taking place in the appearance, disappearance, and variation in distribution and abundance of individual species. One or two species which we should hardly have expected to meet with are included in the list, such as *Lycæna coryden*, but we are surprised to miss not only such species as *Aporia cratægi* (which was common round London at the beginning of the century, though probably no Harrow records were kept so far back), but to find no Fritillaries recorded, except *Argynnis selene*, *euphrosyne*, *paphia* and *Melitæa aurinia*. The fondness of *Vanessa atalanta* for fruit is noticed; and we may remark that *V. antiopa* also shares this habit with its congener.

Altogether, we have to congratulate the authors and the Harrow School Scientific Society on having produced a very creditable little book, and we hope that it will serve as an incentive to the members of other School Scientific Societies to go and do likewise. W. F. K.

Hand-list of Herbaceous Plants Cultivated in the Royal Gardens, Kew. (Sold at the Royal Gardens, Kew.)

ABOUT a quarter of a century ago, the border-flowers in which our grandsires delighted were all but pushed out of existence by "bedding plants" and ribbon-borders of glaring hue. Nurserymen who had good stocks of the older favourites found them unsaleable, and discarded them accordingly. Then came a change, largely owing to the influence exerted by Mr. Robinson's publications. "Herbaceous" and "Alpine" plants were once more received into favour, and are probably more numerous and more extensively cultivated than ever they were. Kew, as usual, has been responsive to popular demands. In times well within the memory of the present generation, the plants we speak of were grown there, as in other botanic gardens, in ugly gridiron-like beds, an arrangement which might have been suitable for strictly botanical purposes, but which was as unattractive as possible.

To obviate this, and to allow of the plants growing in the most natural way possible, the new rockery was formed, mainly, we believe, after the plans of Mr. Dyer. At any rate, it now forms one of the most attractive features in the garden, and with the frames and "Alpine House," serves excellently to illustrate this class of plants.

A proper catalogue, of course, became necessary, for, unfortunately, the names and descriptions in the most popular books on the subject, are not to be depended upon. The present publication is an alphabetical list, the only information given in addition to the names, being a mention of the botanist responsible for the name, and a general indication of the native country of the plant.

The names of the botanical authorities are given in the contracted form adopted in scientific works; but in a list of this character, which is mainly intended for unscientific readers, the names should either be given in full, or an explanation of the abbreviations supplied.

No fewer than 6000 species, it appears, are now grown at Kew, including, we see, as many as a hundred species of Carex.

A Manual of Book-keeping. By J. Thornton. Pp. 527. (London: Macmillan and Co., 1895.)

THE late Prof. Cayley is quoted by the author to have said of book-keeping, "It is only its extreme simplicity which prevents it being as interesting as it otherwise would be." But what was simplicity to the master of pure mathematics is very far from being so to the average shopkeeper, as witness the testimonies of Official Receivers in Bankruptcy. As Mr. Thornton points out, a general opinion among uneducated tradesmen is that book-keeping was invented to conceal the facts; and therefore they think the least they know about it the higher is their code of commercial ethics. This book will undoubtedly assist in removing such mistaken opinions; it is the clearest exposition of the principles and practice of book-keeping that we have yet seen, and the most original in design. The science and art of the subject are dealt with simply; the matter is arranged in an admirable manner; and by subordinating details to principles, the author has made his book worthy of the attention of all students who wish to acquire a sound and scientific knowledge of book-keeping.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The University of London.

MR. THISELTON-DYER now narrows his attack to my suggestion that in voting on the new Charter, members of Convocation should do so "as at a Senatorial election," *i.e.* by voting papers. This seems a very narrow basis for so severe a condemnation.

The reason for this provision was, I presume, that as many members of Convocation are professional men, masters of schools, &c., it is in many cases difficult, if not impossible, for them to come up to London.

The provision applies, I may add, not only to Senatorial, but also to Parliamentary, elections. I cannot see why Mr. Thiselton-Dyer should assume that a vote so taken would "destroy the prospects of academic study in London." That, however, is not an attack on me, but on the Constituency.

High Elms, August 17. JOHN LUBBOCK.

Plant-Animal Symbiosis.

IN Prof. Stewart's collection at the Royal College of Surgeons there is a preparation of a mimosa which protects itself from browsing animals by providing in its great thorns a domicile for a species of vicious, stinging ants. I believe this example of plant-animal symbiosis comes from one of the West Indian Islands, while on the mainland of America the same species of mimosa exists, but suffers greatly from the depredations of animals, because there is no suitable ant to come and ward them off. If my recollection of the distribution is correct, the following note of a similar phenomenon in South Africa, I think, is of considerable interest.

In a recent tour through the Karroo, in search of the skeleton of the Dicynodons, I came across a mimosa tree which here forms the chief fuel, on one of the lower branches of which there were some very large thorns; one of these had a little oval hole bored just beneath the summit. On breaking it open, there issued an incredible number of ants, considering that they were packed in the space of a pair of spines about four inches long and half an inch in diameter. The asexual forms were of the usual two kinds: the soldiers were about a quarter of an inch long, brown, and very attenuated, showing very markedly the influence of surroundings on form; while the workers were scarcely half the size of their protectors, and of a darker hue. The sexual forms I did not see. The ants emerged from the crack in a very sleepy manner, and did not seem at all aggressive; this may have been on account of the cold, which would affect them more than their relatives which live in the earth. Embedded in the soft wood of the stem, where the two spines meet, were several aphides, which thus were able to feed themselves on the sap of the tree, and yet always be within the house of their owners. In the West Indian thorn-tree the leaves offer a further inducement to the ants to remain constantly near them, by providing at the extremity of the leaflets little masses of a nutritious substance adapted to the digestions of their guests; in the South African tree there is a mass situate at the base of the leaves, similar to that in the cherry, which probably serves the same object. On returning shortly afterwards, I found the ants had trekked with all their cattle, and I failed to trace their whereabouts. The locality was the gold-fields of Spreeunfontein, in the Prince Albert district. ERNEST H. L. SCHWARZ.

Cape Town, August 1.

Definitions of Instinct.

I HAVE read with interest the abstract of Mr. C. W. Purnell's paper which you published in last week's NATURE (p. 383). I think he is in error in supposing that young birds do not afford us examples of truly instinctive activities. The way in which a young moorhen swims with accurate coordination, before the down is well dry after hatching, and before it can walk steadily, is very instinctive. I would suggest to Mr. Purnell that there is a wide field for observation open to him among his native birds. If he will hatch some of them out in the incubator, and carefully note what they can do prior to experience, and how their activities are modified by experience, he will help to solve some of the difficult problems of habit and instinct.

I have myself advocated a restriction in the meaning of the term somewhat similar to that for which he argues. I shall be obliged if you can find space for the provisional scheme of terminology thus suggested in *Natural Science* for May 1895, which I have since somewhat extended and amended. To bring it into line with modern biological thought, a good deal of stress is laid on the question of heredity, and on the distinction between the definiteness which is congenital and that which is acquired. It may be premised:

(1) That the terms *congenital* and *acquired* are to be regarded as mutually exclusive. What is congenital in its definiteness is, as prior to individual experience, not acquired; the definiteness that is acquired is, as the result of individual experience, not congenital;

(2) That these terms apply to the individual. Whether what is acquired by one individual may become congenital through inheritance in another individual, is a question of fact which is not to be settled by implications of terminology;

(3) That the term *acquired* does not exclude an inherited potentiality of acquisition under the appropriate conditions. Such inherited potentiality may be termed *innate*. What is acquired is a definite specialisation of an indefinite innate potentiality;

(4) That what is congenital and innate is *inherent* in the germ-plasm of the fertilised ovum.

Congenital movements and activities: those the definite performance of which is antecedent to individual experience. They may be performed either (1) at or very shortly after birth (*connate*), or (2) when the organism has undergone further development (*deferred*).

Congenital automatism: the congenital physiological basis of those movements or activities the definite performance of which is antecedent to individual experience.

Physiological rhythms: congenital (and connate) rhythmic movements essential to the continuance of organic life.

Reflex movements: congenital, adaptive, and coordinated responses of limbs or parts of the body: directly evoked by stimuli.

Random movements: congenital, more or less definite, but not specially adaptive movements of limbs or parts of the body; either centrally initiated or directly evoked by stimuli.

Instinctive activities: congenital, adaptive, and coordinated activities of relative complexity, and involving the welfare of the organism as a whole; specific in character, but subject to variation analogous to that found in organic structures; similarly performed by all the members of the same more or less restricted group, in adaptation to special circumstances frequently recurring or essential to the continuance of the race; often periodic in development and serial in character.

Imitative movements and activities: due to individual imitation of similar movements or activities performed by others.

Impulse (Trieb): the affective or emotional condition, whether congenital or acquired, under the influence of which a conscious organism is prompted to movement or activity, without reference to a conceived end or ideal.

Instinct: the congenital psychological impulse concerned in instinctive activities.

Control: the conscious inhibition or augmentation of movement or activity. While the power of control is innate, its special mode of application is the result of experience, and therefore acquired.

Intelligent activities: those due to individual control or guidance in the light of experience through association (voluntary).

Motive: the affective or emotional condition under the influence of which a rational being is guided in the performance of deliberate acts.

Deliberate acts: those performed in distinct reference to a conceived end or ideal (volitional).

Habits: organised groups of activities, stereotyped by repetition, and characteristic of a conscious organism at any particular stage of its existence.

Acquired movements, activities, and acts: those the definite performance of which is the result of individual experience. Any modifications of congenital activities which result from experience are, so far, acquired.

Acquired automatism: the individually modified physiological basis of the performance of those acquired movements or activities which have been stereotyped by repetition.

C. LLOYD MORGAN.

A Scheme of Colour Standards.

THE confusion which has long prevailed, and does not promise any immediate disappearance, in the use of colour names, is an inevitable consequence of the absence of any definite standards of colour. In music and form we have well-established and very satisfactory terms to describe definite sense perceptions, and it would be difficult to conceive how we could dispense with them; but for colour perceptions we have neither any well-defined concepts for those terms which have become well established, nor any definite and well-arranged system of colour terms for common use. Those terms which have acquired a somewhat definite significance are nevertheless used for a very wide range of variation. Vermilion and ultramarine, terms which have been used by many of our best authorities on colour, for want of anything better, as a basis for comparison and analysis, are nevertheless used for very variable concepts. The difference between a Chinese and a German vermilion in pigments is very noticeable. A Winsor and Newton "chrome yellow" and a German "chrome yellow" differ by more than twenty-five per cent. of yellow. Among several samples of blue pigments a still greater variation is generally found. When this is true of such terms, what shall we expect will be the case with that very much larger group of terms whose meaning has never reached any considerable degree of accuracy, as olive, citrine, russet, &c., or that still more vague but innumerable class of terms in vogue in popular usage, like "crushed strawberry," "baby blue," "ashes of roses," "peacock blue," "hussar blue," and a host of others still more vague and transitory?

Naturalists have been at very great inconvenience because of the lack of any agreement in the use of colour terms for botanical, entomological, and ornithological descriptions. Our greatest American authority in descriptive botany is sometimes confusing in his description of flowers because of this lack of any standard terms with admitted significance. Zoologists, too, have sought in vain for some basis of agreement, and the futile attempts to establish some such basis of agreement are familiar to every ornithologist.

In applied science and the arts the inconvenience has, if possible, been still greater, inasmuch as the number of persons interested is larger. And this inconvenience is steadily increasing as the revelations of chemistry disclose hues more and more brilliant, for which new names are as constantly coined. With the rapid advance of the art of dyeing the necessity of some system of colour nomenclature becomes more and more imperative.

The valuable research of Prof. Rood has contributed greatly to our knowledge of colour, and that in a time when much less was known on the subject than now. The later contributions of Abney and Church in England have also given valuable additions to the science of colour. The able works of Chevreul, Koenig, and Von Bezold are also important contributions to the subject. Above all, the masterly works of Helmholtz, Hering, Kühne, and Carpentier are most valuable contributions upon this subject.

But while these have given the greatest help to our understanding of the nature and relations of colour, none of them has given any solution to the problems just now hinted at. No system of colour nomenclature has been offered, nor any set of colour standards proposed.

It was in view of these difficulties that the writer proposed, about twelve years since, while connected with the Springfield, Massachusetts, High School, as teacher of physics, botany, and zoology, that a series of colour standards, based on the hues of the solar spectrum, and selected by a consensus of colour experts, should be adopted as a foundation of all our colour work, and especially that its use in all educational work be made the means of establishing a better and more accurate knowledge of colour. This proposition was received with favour from the first by those to whom it was mentioned, and an attempt was soon made to put the scheme into material form; but the difficulties which lay in the way of producing any pigmentary hues which could in any adequate degree represent the hues of the solar spectrum were so great, that very little progress was made for several years.

The wave theory of light long ago established the fact that vibrations of an almost infinite variety of wave-length in the luminiferous ether impinge upon the human retina and produce the effect which we call white light. From these we may select any wave-length we please, and giving it a name, have a colour as accurately fixed as any musical note or geometrical form. The desirability of such a definiteness in the terms which describe colour, all will immediately recognise. It is not necessary that a large number of thus accurately fixed colours should be made the basis of colour nomenclature, for then the eye could not distinguish between the colours selected. Every eye can, however, unless it be colour-blind, distinguish six well-marked colours in the solar spectrum, for which there are well-recognised names. The theory of three primary colours from which all the other hues of the spectrum are derived is no longer possible with the present knowledge of the laws of light, and the always questionable indigo of the rainbow is no longer recognised as one of the distinct spectrum colours. As a matter of practical convenience we may select any number of colours which is found to be desirable for standards if only they are accurately defined. This has been done for the six commonly recognised spectrum colours, red, orange, yellow, green, blue, and violet.

To obtain the agreement of six or eight persons well skilled in the use of colours as to exactly what portion of a projected spectrum of eight or ten feet long should be selected for each standard was a much less difficult task than would first have been supposed. It was found that very great unanimity of judgment prevailed when the comparison was made. The portion of the spectrum having been selected for each colour, two other things yet remained to be done. These were very important factors of the proposed scheme.

First of all the exact location of the area must be determined by careful measurement of the wave-length of its centre. This would make it possible to ascertain or relocate the standard in any part of the world without any material representation of the colour designated; in other words, this makes possible the use of the designated colour as one of a series of universal standards. To render any set of standards of most permanent and wide value, it is desirable that it be adopted by somebody whose authority will be generally recognised. In the case of standards of measurement, the Government establishes standards in the interests of commerce and equity. In most other cases some learned society adopts the standard, and thus gives it the benefit of its own authority. The American Metrological Society has appointed a committee with instructions to report a recommendation for the establishment of six standard colours. The endorsement of such a society would go far toward the establishment of any scheme of colour nomenclature if the scheme be a practical one. Without the element of practicability no authority could make such a scheme of any value.

After several years of careful study of the practical problems involved, especially the relations of the colour standards to one another, it was found that to select any particular pigment as one of the standards, and make the scheme adapt itself to this as a standard, was not only unscientific but unpracticable. The standards if selected with a view to practical or artistic purposes, and most of all with the hope of making the scheme of educational value, must bear a relation to one another not unlike those of the musical scale. It must be possible at least that the union of these standards should produce the intermediate hues of the solar spectrum in colour if not in purity. After much care six standards were selected and at once put to practical use. This was as early as 1884. The exact measurements of the wave-lengths of these standards were published in *Science* for June 9, 1893. The values there given were as follows: Red 6587, orange 6085, yellow 5793, green 5164, blue 4695, violet 4210, in ten-millionths of a millimetre.

These measurements are for the centre of an area of the solar spectrum represented by fifty of the same units. A measurement differing from either of these by twenty or twenty-five would hardly vary to a degree to be perceptible to the trained eye, much less to the ordinary eye. There is, however, a very great variation in different parts of the spectrum. In the orange, yellow, and green, where the change is rather rapid, a small difference is very readily perceived, while in the red as well as in the blue and violet the same difference would be scarcely noticeable.

To Mr. Milton Bradley, of Springfield, Massachusetts, is due great credit for first undertaking to put this idea of spectrum standards into practical working material. When the idea was first proposed he was largely engaged in the manufacture of coloured papers for educational purposes, and he at once undertook to reproduce the spectrum colours in his educational papers. The task proved no easy one, notwithstanding the great advance which the discovery of the aniline dyes had made in the production of brilliant colours. It was only with the utmost persistency that Mr. Bradley was able to accomplish the task which he had voluntarily undertaken. After long and repeated experi-

ments he succeeded in getting coloured papers which are very good reproductions of the hues of the solar spectrum. These papers have now been used for several years very extensively in kindergarten and primary school work, and they are an important means toward the education of a new generation of students to a true conception of colour, a more careful use of colour terms, and a sharper discernment of colour perceptions.

At a meeting of the Society of American Naturalists, held in Boston, December 31, 1890, I read a paper in which was given a more elaborate carrying out of the scheme which I had previously proposed.

In order that any fixed scheme of colour nomenclature may be of some practical value it must, of course, be readily understood by people of only ordinary intelligence, and must be complete enough to meet the ordinary wants of everyday life. There must be something that is so completely fixed as to be perfectly trustworthy for present and future needs.

In the solar spectrum we have an invariable source from which to derive our spectrum standards, and upon these the whole scheme is to be based.

Since, however, the six spectrum standards do not give a very extensive *répertoire* for common use, to say nothing of the needs of the more artistic, it was proposed to introduce between each two spectrum standards two intermediate hues to be formed by the union of the two spectrum standards in definite proportions. Thus between orange and red would be introduced an orange-red and a red orange. In the former red would predominate, while in the latter orange would be more prominent. Inasmuch as these hues are only intended to be combinations of the spectrum standards, it is not necessary, or even perhaps desirable, that these hues be absolutely fixed. If, however, this is desirable in any particular case, it can be accomplished in a manner which will be indicated subsequently. In addition to the two hues introduced between each two standards it is also necessary to use a violet-red and a red-violet (or two purples, a reddish purple and a violet purple) to represent the actual combinations which occur in nature.

It is also very desirable that the standards be produced in some material form in order that it be of any practical value. The task of reproducing the brilliant hues of the solar spectrum in pigmentary material or in glass is much more difficult than one not acquainted with the matter would suspect. It would not be difficult to select well-known pigments, and then determine the wave-length which most nearly corresponds to the hue of the pigment; but any number of such selections would not form a symmetrical series of colour standards. The colours for such a scheme being selected and their wave-length determined, the other and more difficult problem is that of finding some combination of pigment which will reproduce it. This task of reproducing the spectrum hues was a very difficult one. It is impossible to reproduce some of the spectrum colours with the ordinary pigments either in hue or in quality.

Almost at the very outset of this work in colour it was found that it would be necessary to depend upon the somewhat fugitive aniline colours for some of the standards as the only colour material which would approach the spectrum hues in brilliancy. The difficulty of keeping the standards up to tone, so to say, while using somewhat changeable material, is a serious inconvenience but not an insuperable barrier. With the solar spectrum recognised as the source to which we must always go to correct our standards, the great difficulties of colour-study are met. The most desirable thing now to be accomplished is the discovery of some permanent colour material in which to reproduce the spectrum standards. Some convenient form of tablet would then be produced which could be supplied to all who are willing to provide themselves with it, and to these all questions of colour would be referred. The standards thus established, the intermediate spectrum hues are determined by them.

Now, by the use of the Maxwell discs in the standard colours described above, we may fix upon definite proportions of each which we will use for any other hue. If, for example, we desire to introduce between red and orange two hues, we must first of all know something of the relative effect of the two colours, and combine them in inverse proportion to what we are accustomed to call the value of the colours. The colour which has the lowest value will require to be used in larger proportion than the other. In this case we may take a red and an orange disc and put them together in the manner above described. For convenience of measurement, a disc just a little larger than the coloured discs, with the margin graduated into one hundred degrees, is placed behind the coloured discs, and the sectors adjusted as desired. As the red has the lowest value, more of the red disc must be exposed in order to produce an effect equal to that produced by the orange. If it is desired, therefore, to introduce two hues between red and orange, we must still more increase the proportion of red in the combination which we wish to be most like the red. For our orange-red we may use 70 per cent. of red and 30 per cent. of orange, and for our red-orange 59 per cent. of red and 41 per cent. of orange. By making a scale of values for the six standard hues, we may combine them in the manner we have just illustrated and form two hues between each of the standards, and two more by combining red and violet. These twelve hues, with the six standards, give a sufficiently large variety of hues for practical purposes.

For purposes of colour education, however, it must be borne in mind that pure spectrum colours are not often seen either in nature or art. And while it is very important that the student should be taught the spectrum colours at the outset of his education in order to establish some accurate knowledge, derived from the only source of accuracy, the solar spectrum, it is also important that he should become familiar with the effect produced by the mingling of these spectrum hues with the light reflected from other objects, as well as the effect of shadow upon the colours themselves. The mingling of white light with any colour produces a tint of that colour. The tints are what we most often see in all except the most brilliant colours of flowers, not generally of the standards but the intermediate hues. On the other hand, when a coloured object is seen in shadow, or, what is more common, when the coloured surface is so irregular as to reflect here colour and there give no reflection, the effect is to produce a shade of the colour. In foliage the prevalence of shades is the rule, whether we consider the individual leaves or the masses of foliage. A knowledge of these effects is best acquired by the use of a very few tints and shades of each hue. Any convenient number of tints and shades can of course be designated, but a few will serve all the purposes of ordinary educational work. In their educational papers the Milton Bradley Company use the six spectrum standards, twelve intermediate hues, including the combinations of red and violet, two tints and two shades of each of the pure colours, thus giving in all a range of ninety different modifications of colour. With these are used black and white, together with a variety of greys. The facility with which young children learn to distinguish and designate colour is really quite surprising.

But a still larger proportion of the colour effects of nature and art than those produced from either tints or shades are the result of both light and shadow combined with colour. This effect has been well enough described by the term "broken colour." In order to acquire familiarity with this effect, it is desirable to use a series of broken standards, if not also of the twelve intermediate hues. These should be made, as should also the tints and shades, by using proportions which take into account the value of the colours, and, above all, the proportions of white and black used should be such as to avoid destroying the characteristic effect of the colours. Each of these broken standards may have its tints and shades like the standards themselves by increasing the amount of white or black which is combined with the colour.

But perhaps the most interesting point in connection with the introduction of definite colour standards will be the possibility of talking about colour in a definite language. Without such standards this has been impossible. By the use of the Maxwell discs made in the standard colours we may easily determine the composition of any colour. This is a great convenience in the description of colours, for it renders it possible when it is necessary to give an exact meaning to any colour term. Only with standards which can be accurately fixed is this possible. The use of such terms as vermilion, emerald green, ultramarine, chrome yellow, and similar terms as a basis of colour analysis is exceedingly impracticable, since even these terms, although by far the most definite terms in common use, are quite too variable to give results which can be of any real value. For the sake of convenience, the first letter of each colour is used as the symbol of the colour in all formulæ in which the analysis or composition of colour is expressed. N is used for black, to avoid the repetition of B which is used for blue.

The following formulæ will illustrate the practical application of the idea and the value of the spectrum standards in determining the composition of colours. They will also be of interest as

showing the simplicity of the proposed nomenclature and method of expressing the results of analysis.

The first series illustrates the variability of the pigments used by artists. These analyses are made by Mr. Bradley.

A Winsor and Newton "cinnabar green" gives—Y 14, G 11½, N 74½.

A German pigment of the same name gives—Y 12½, G 11, W 2, N 74½.

A Winsor and Newton "light red" gives—O 24, N 76.

A German pigment of the same name gives—O 18, N 82.

A Winsor and Newton "chrome yellow" gives—O 29, Y 71.

A German pigment of the same name gives—O 35, Y 45, N 20.

A Chinese vermilion gives—R 77, O 23.

A yellow ochre gives—O 24, Y 24, N 52.

An Indian red gives—R 7½, O 17½, N 75.

An emerald green gives—G 63, B 14½, N 22½.

One called "chrome green" No. 2 gives—G 16½, Y 55, N 78½.

The following series illustrates the significance of the terms used in describing the colours of dress goods. A very wide range of tints and shades of the colour which is the basis of each term will often be designated by the same name.

A sample of goods called "écru" is—O 11, Y 13, W 18, N 58.

Another sample marked "raisin" gives—R 18, V 14, W 5, N 63.

A sample called "ashes of roses" gives—R 8, V 4, W 14, N 74.

The popular colour called "eminence" gives—R 14, V 19, N 67.

Another popular colour called "emerald" is—G 21, B 3, N 76.

A sample called "crushed strawberry" gives—R 55, O 5, W 27, N 11.

One having the poetic name "absinthe" gives—Y 35, G 45½, N 19½.

Another called "Marion" gives—R 4, O 3, N 93.

A specimen of "hussar blue" gives this—G 4, B 15, N 81.

A sample called "oasis" gives the formula—Y 7, G 10½, W 8½, N 74.

Another called "dove colour" gives—B 9, W 9, N 82.

Still another, called "prairie," gives—Y 10½, G 14½, N 75.

A colour called "Styx" has this formula—R 9½, W 21½, N 69.

A sample of "peacock blue" gives this—G 4½, B 8½, N 87.

A brown, called "vidette," gives this—O 4½, Y 3, N 92½.

A sample of "navy blue" gives—B 6, N 94.

Another of "Turkey red" gives—R 98, O 2.

A rather dark "plum colour" gives—R 3, V 4, N 93.

A few analyses of flowers will be of interest to others beside the botanist.

The Fringed Polygala (*P. paucifolia*) is—R 48, V 52.

The Wistaria (*W. frutescens*) gives—for the wings R 11, V 89; and for the standard R 9, V 79, W 12.

The Flowering Quince (*Cydonia japonica*) gives—R 95, V 2, W 3.

The wild Cranesbill (*Geraneum maculatum*) gives—R 28, V 66, W 6.

The Fosythia (*F. viridissima*) is pure spectrum yellow.

The variations of foliage are worthy of note, and a few examples of analyses of the colour of various leaves will perhaps be of interest.

It is possible that some knowledge of these variations on the part of more of our artists might save us some of the abominable greens which so often appear in paintings, otherwise of an excellent grade.

Leaves of the White Oak give—Y 7½, G 11½, N 81.
" " Apple are—Y 5, G 13, W 2, N 80.
" " Copper Beech give—R 17, V 2, N 81.
" " Hemlock Spruce—Y 2, G 9, N 89.
" " White Pine give—Y 2½, G 11, N 86½.
" " White Birch give—Y 5½, G 11½, W 1, N 82.
" " Hornbeam—Y 5½, G 12½, N 82.
" " Shagbark Hickory—Y 4½, G 9½, N 86.

With discs made in the spectrum standards colour can thus be analysed and the results, expressed as in the examples just given, can be utilised by any number of persons to determine the particular colour about which a statement is made. As these discs are not expensive, and the means of rotating them very simple, they ought to come into very general use. It is only necessary that they be rotated with sufficient rapidity to cause the colours to blend smoothly. For the purposes of studying the harmony and contrast of colour it is desirable to have discs of several sizes, so that two or three combinations of colour may be made upon the colour-wheel at the same time and compared.

Among the practical applications of such a scheme of spectrum standards as that outlined in the preceding paragraphs, some of the most obvious are the only ones which need be mentioned in this connection.

A firm dealing in large quantities of coloured material desires to order a stock in a particular colour which they have not used, and of which they have therefore no samples. By the old method they must find something as nearly like what is desired as possible, and then dictate as best they can just what variations are to be made. Now they can produce the colour with the discs and send the formula only to their manufacturer, who also has a set of the discs, and he "sets up the colour" and then reproduces it in the material desired. The gain is great in several ways. In the first place it saves the dealer much costly experiment to determine just what he really wants. Again, if he is in doubt as to just what a customer wants, he takes him to his colour wheel and ascertains what the desired colour is, and then communicates it to the manufacturer. The architect may spend much time and effort to have his carefully-planned and beautiful villa painted in colours which will be at once in keeping with the style of architecture and the surroundings of the building; but unless he confine himself to colours ready prepared and of certain composition, he is liable to extreme disappointment. A similar use of the colour wheel with standard discs would greatly reduce his difficulties. The artist who accustoms himself to the analysis of colour effects will soon find that he is able to write estimated formulæ which will be of service to him in the subsequent composition of his observations. Above all, the child who is thoroughly educated in any scheme of colours which has a definite basis, and consists of a well-selected series of standards, is starting with a most valuable groundwork for future knowledge and practice. Hence it is that the introduction of systematic colour work into the kindergarten and primary school has so much of encouragement to those who desire a reformation in the use of the terms which describe colour perceptions. Why may we not hope for the time when a system of colour terms with something of the same definiteness as those used in music shall be in common use? Surely there is need of this, and the time is not far distant when this need will so assert itself as to bring about a revolution in our methods of colour education.

Malden, Mass., U.S.A. J. H. PILLSBURY.

Globular Lightning.

ON June 21, about 6 p.m., Dr. Wallis, Mr. Taylor and myself were in our drawing-room on the ground floor, taking shelter from a passing storm; they were seated, and I stood five paces from them. The doors were all closed against the storm, and I went out and, for cool air, opened one. On returning, I saw a globular light, about the size of the full moon, in the air between Wallis and Taylor, and almost instantly I heard in the room a terrific clap of thunder like a cannon. I suffered afterwards from acute pain down the left side of my face. Taylor, who had an iron-headed golf stick in his hand, felt a twinge up his right arm, and a sensation as of singeing in his hair. Wallis felt nothing at all. We all experienced a sulphurous smell. In the adjoining room, leaning against one corner, were two Martini-Henry rifles in leather cases. One was untouched. The stock of the other was almost shattered, splinters lying about the room. The leather covering of the splintered rifle was torn, but the metal part of the rifle quite unhurt. At the point of the wall where the muzzle of the shattered rifle touched the wall, there was a hole 5 × 2½ and 1½ to 2 inches deep. The wall is of mud and plaster. In the room above were two holes in one wall; that is, the wall above that in which the hole appeared below. These holes were smaller than the one below. Just below the two holes stood a wooden case, iron-bound, and at its foot the matting was torn up, but the floor and the case were untouched. In the second room above, that is, the room over that in which I had seen the globular lightning, the wall near the ceiling was cracked for six or eight feet. This was all the damage done that we could find. G. M. RYAN.

Karachi, July 18.

[The above letter was received from Mr. F. C. Constable, who saw the damage described.—ED. NATURE.]

RECENT STUDIES ON DIPHTHERIA.

IT is an acknowledged fact that as regards diphtheria, personal predisposition on the part of its victims plays a most important part.

We find this well illustrated by statistics which show that it is in early childhood that the majority of cases occur, and the heaviest diphtheria death-rate is recorded. Thus Feer in Basel found that the most susceptible age to diphtheria lies between the years 2 and 5 and 5 and 10; but that whilst the mortality amongst children attacked in the *earlier* period was 25·4 per cent., in the *later* period, with practically no diminution in the number of cases, the diphtheria death-rate fell to 7·6 per cent. After this period there is not only a great decline in the number of cases of diphtheria, but also a marked decrease in the percentage of deaths, suggesting that with increasing age the human system is enabled gradually to develop means of protection from this terrible disease.

That some such protective power must also be possessed to a large extent by children, follows from the fact that with a disease practically endemic in some of our large cities, so many children succeed in escaping from its ravages, for it is impossible to conceive that all those who have remained unscathed have never been exposed to infection from diphtheria.

Thus Flügge has worked out an interesting diphtheria-table for the city of Breslau during the years 1886–1890, in which he not only confirms Feer's observations upon the connection between age and the diphtheria death-rate, but he also shows very clearly that even in the most susceptible period of child-life, the number of cases of diphtheria is relatively small when compared with the number of children of the same age who are not attacked.

In what does this protective power against diphtheria infection possessed by many children and a large number of adults consist? This interesting and important question Dr. Wassermann has recently endeavoured to answer by making a very extensive examination of the properties possessed by the blood serum derived from patients not suffering from diphtheria, but admitted on other grounds to the Berlin Institute for Infectious Diseases. Careful inquiries were, moreover, in every case made as to the patient's previous history as regards diphtheria, and only those were included in the investigation who had never had diphtheria.

The serum which was obtained from these strangers to diphtheria was in every case tested for its immunising or protective power by inoculating it along with a recognised lethal dose of diphtheria toxin into guinea-pigs, the latter by itself having been proved capable of killing these animals without exception in from 30 to 48 hours.

The results obtained were extremely interesting. Out of seventeen children varying in age from 1½ to 11 years, eleven yielded serum with highly protective properties as regards diphtheria, for all the animals treated with their serum and virulent diphtheria toxin experienced no ill-effects whatever. Two out of the seventeen children yielded serum possessed of slightly protective power, it being found capable of delaying the death of the infected animals, whilst the serum derived from the four remaining children had no protective properties whatever.

Amongst the adults the number of those yielding an anti-toxic serum was much greater, for out of thirty-four individuals the serum of as many as twenty-eight was found to be endowed with protective properties against diphtheria infection; and, as far as the investigation went, it appeared that the possession of such serum, as well as its strength or degree of efficiency, was more marked with increasing age.

That people who have gone through the ordeal of diphtheria possess such antitoxic serum in their system has been shown by various investigators, but, so far as we know, Wassermann is the first who has proved that anti-diphtheritic serum may also be possessed by individuals who have had no previous experience of diphtheria.

This discovery serves to explain how virulent diphtheria bacilli may be present in the throat of perfectly healthy people, without producing any bad results at all. That such may be the case has been proved by most careful and trustworthy observers, and that their presence does not engender diphtheria, we must now regard as probably due to the possession of anti-diphtheritic serum by the individual who so unconsciously has harboured them. Such may also be, and probably is, the explanation of the harmless presence of virulent diphtheria bacilli in the throats of patients convalescent from diphtheria long after the disappearance of all the typical symptoms.

It does not follow, however, that because at some given time a particular individual has been found the happy possessor of anti-toxic serum he may, therefore, rashly assume that he is for ever after proof against diphtheria infection.

It must be remembered that such serum is possessed in very different degrees of strength by different individuals, and may vary also, in one and the same individual, in its protective character at different times.

Research has shown that people possessing only feebly antitoxic serum can contract diphtheria, but in the majority of such cases it is satisfactory to learn that the symptoms are light, and the disease is mastered without much difficulty.

So far as our present knowledge goes, it would appear reasonable to admit that although the possession or non-possession of antitoxic serum of varying degrees of strength may not be the only circumstance which regulates the fluctuating personal disposition towards diphtheria infection, that yet it may be regarded as an important factor, and Wassermann considers principal cause, in determining the apparent idiosyncracies of diphtheria infection. What the mechanism may be whereby this anti-toxic serum is produced in the system is still a mystery; that it should be possessed by infants only eighteen months old, would incline to the belief that it is natural or inborn, and not subject to later processes of evolution.

On the other hand, however, we have the well-established fact that the serum of animals which have a natural or race immunity to a particular disease, is wholly devoid of power to confer protection from this disease on other classes of animals.

This remarkable circumstance has been once more very clearly demonstrated by Wassermann in the case of diphtheria, to which disease white rats are absolutely immune. In order to test the character of white-rat-serum as regards diphtheria infection, fatal doses of diphtheria toxin were administered to guinea-pigs along with such serum, but in no single case did the latter survive, showing that this serum possessed no anti-diphtheritic properties whatever, and was incapable of protecting animals from diphtheria infection.

Thus, on the one hand, we find that natural or race immunity to a particular disease does not provide protective serum against infection from that disease in other animals, and, on the other hand, that the serum of individuals who have never had diphtheria, does provide in many cases such protective serum.

Now Wassermann argues from these facts that the possession of protective human serum is not natural or born with the individual; for otherwise, as in the case of white-rat-serum, it would be incapable of conferring immunity, that it must therefore rather be regarded as a later acquisition, and subject to evolution processes.

In pursuing this line of reasoning, Wassermann assumes that race immunity found to be characteristic of a particular description of animal is necessarily of the same character as exceptional immunity confined to particular

individuals of a race. In the one case it belongs to the whole race, whilst in the other it is possessed by only particularly fortunate individuals of a race.

Does not this point rather to the operation of exceptional circumstances, in which, possibly, heredity may play a part? How is it that whereas some families appear to have a faculty for contracting every zymotic disease, others exposed to similar conditions, have an equally characteristic faculty for escaping such diseases?

The impression is irresistible that such a faculty is born with or natural to the individual.

It may be argued that the white-rat-race-immunity may also be ascribed to the operation of heredity. This is quite possible, but in the one case the immunity is perfected or heredity has accomplished its work, whilst in the other it is incomplete and is still in an evolutionary stage. The race immunity to diphtheria, or immunity in its perfected condition, is evidently of a different order, and may also very possibly have been developed on quite different lines, from that which we have been discussing in the human subject. In what this difference consists is at present unknown, and until we have a more intimate understanding of the actual condition in the system upon which immunity depends, or a closer insight into the particular agents responsible for its production we cannot hope to arrive at any definite conclusion.

There is, however, another obstacle to a logical acceptance of Wassermann's arguments as to the origin of protective diphtheritic serum in the human system, that is to say, in the light of our present knowledge, for it entails the supposition that such individuals have been subjected to the action of diphtheria bacilli. This supposition is the logical outcome of the bacteriological evidence which is at our present command on this subject. Thus it has been found, over and over again, that the serum of animals artificially rendered immune to a particular disease, is only efficacious in affording protection to other animals infected with *identically the same microbial disease*. This has quite recently been carefully worked out by Pfeiffer, who has shown that the serum of horses rendered immune to cholera is only efficacious in cases of infection from the cholera vibrio, and that it is absolutely inoperative in protecting from an infection due to any other vibrio, however nearly the latter may resemble that of the cholera vibrio.

But we have seen that protective serum may be possessed by individuals who have never had diphtheria, on whom, moreover, careful investigation has not been able to reveal the invariable presence of true diphtheria bacilli. So far it must be acknowledged, then, that we have no working hypothesis which enables us to comprehend aright the circumstances which determine the presence of or control the generation of anti-diphtheritic serum in the human system, and we are therefore powerless to either stimulate or diminish its production; but we are, however, in a position to regulate, to a great extent, the dissemination of diphtheria virus from one individual to another.

It has recently been shown that children taken from diphtheria surroundings, and not themselves suffering from the disease, in a large number of cases carry about with them in their nasal and throat passages typical virulent diphtheria bacilli, and that although they do not necessarily themselves develop the disease, they thus become the dangerous carriers of infection.

It is considered essential, therefore, that no member of a family where diphtheria has occurred, should be allowed to mix with others until a bacteriological examination has shown that diphtheria bacilli are absent from the air passages, neither are those who have recovered from this disease to be permitted to resume their usual occupations until the absence of diphtheria bacilli has been conclusively proved.

In Germany such systematic examinations are rapidly gaining ground, and already in some of the hygienic institutes the practice is regularly carried out. Indeed, in Königsberg, von Esmarch has suggested that to facilitate the universal adoption of such precautions, the throat of the patient or suspect should be wiped with a sterile sponge, and the latter forwarded for bacteriological examination.

The causes at present at work contributing to the generation of diphtheria in London have yet to be found.

If the contraction of diphtheria primarily depends upon the presence or absence of anti-toxic serum in the human system, then it would appear that some causes are at work tending to deprive the individual of the capacity to generate this means of protection.

It is difficult to conceive, and hard to realise, that the advance in sanitary science and improved hygienic conditions of the present day have but resulted in London in increased facilities for generating and distributing the virus of diphtheria, and that so far we have proved ourselves hopelessly unable to fathom this problem, or to stay the progress of this terrible malady.

REPORT OF THE COMMITTEE APPOINTED BY THE SMITHSONIAN INSTITUTION TO AWARD THE HODGKINS FUND PRIZES.[1]

THE Committee of Award for the Hodgkins prizes of the Smithsonian Institution has completed its examination of the two hundred and eighteen papers submitted in competition by contestants.

The Committee is composed of the following members Dr. S. P. Langley, Chairman, *ex-officio;* Dr. G. Brown Goode, appointed by the Secretary of the Smithsonian Institution; Assistant Surgeon-General John S. Billings, by the President of the National Academy of Sciences; Prof. M. W. Harrington, by the President of the American Association for the Advancement of Science. The Foreign Advisory Committee, as first constituted, was represented by M. J. Janssen, Prof. T. H. Huxley, and Prof. von Helmholtz; and after the recent loss of the latter, Dr. W. von Bezold was added. After consultation with these eminent men the Committee decided as follows:

First prize, of ten thousand dollars, for a treatise embodying some new and important discoveries in regard to the nature or properties of atmospheric air, to Lord Rayleigh, of London, and Prof. William Ramsay, of the University College, London, for the discovery of argon, a new element of the atmosphere.

The second prize, of two thousand dollars, is not awarded, owing to the failure of any contestant to comply strictly with the terms of the offer.

The third prize, of one thousand dollars, to Dr. Henry de Varigny, of Paris, for the best popular treatise upon atmospheric air, its properties and relationships. Dr. de Varigny's essay is entitled "L'Air et la Vie."

(Signed), S. P. LANGLEY,
G. BROWN GOODE,
JOHN S. BILLINGS,
M. W. HARRINGTON.

August 9, 1895.

SUPPLEMENTARY REPORT OF THE COMMITTEE APPOINTED BY THE SMITHSONIAN INSTITUTION TO AWARD THE HODGKINS FUND PRIZES.

After having performed the function to which the Committee was called, as announced by the circular of the Secretary of the Smithsonian Institution, dated March 31, 1893, which function did not include the award of any medals, there remained several papers to which the

[1] Communicated by Dr. S. P. Langley, Secretary Smithsonian Institution.

Committee had been unable to give any prize, and to which they had felt desirous to give some honourable mention, and on their representing this to the Smithsonian Institution, they had been commissioned to do so, and also to give certain medals of silver and bronze which had been subsequently placed at their disposition.

The Committee has decided that honourable mention should be made of the papers, twenty-one in number, included in the following list, which also gives the full names, titles, and addresses of the authors, and the mottoes or pseudonyms which in four instances were employed. To three of the papers a silver medal is awarded, and to six a bronze medal.

Honourable Mention with Silver Medal.

Mr. A. L. Herrera and Dr. Vergara Lopez, of the city of Mexico: "La Atmosfera de las altitudes y el bienstar del hombre."

Mr. C. L. Madsen ("Geo"), Helsigór, near Copenhagen, Denmark.

Mr. F. A. R. Russell, of London, Vice-President of the Royal Meteorological Society of Great Britain: "The Atmosphere in Relation to Human Life and 'Health.'"

Honourable Mention with Bronze Medal.

Mr. E. Deberaux-Dex and Mr. Maurice Dibos ("Spes"), of Rouen, France: "Études des courents aériens continentaux et de ieur utilization par des párostats long-courriers."

Dr. O. Jesse, of Berlin, "Die leuchtendon Nachtwolken."

Dr. A. Loewy, of Berlin: "Untersuchungen über die Respiration und cirkulation unter verdünnter und verdichteter Sauerstoffarmer und sauerstoffreicher Luft."

Mr. Alexander McAdie ("Dalgetty"), of Washington: "The known properties of atmospheric air considered in their relationships to research in every department of natural science, and the importance of a study of the atmosphere considered in view of those relationships: the proper direction of future research in connection with the imperfections of our knowledge of atmospheric air and the conditions of that knowledge with other sciences."

Mr. Hiram S. Maxim, of Kent, England: "Natural and Artificial Flight."

Dr. Franz Oppenheimer and Dr. Carl Oppenheimer ("E pur si muove"), of Berlin, Germany: "Ueber atmosphärische Luft, ihre Eigenschaften und ihren Zusammenhang mit dem menschlichen Leben."

Honourable Mention.

Mr. E. C. C. Baly, of University College, London: "The decomposition of the two constituents of the atmosphere by means of the passage of the electric spark."

Prof. F. H. Bigelow, of Washington: "Solar and Terrestrial Magnetism and their relation to Meteorology."

Dr. J. B. Cohen, of Yorkshire College, Leeds, England: "The Air of Towns."

Dr. F. J. B. Cordeiro, of Washington:—"Hypsometry."

Prof. Emile Duclaux, of the French Institute, Paris, France: "Sur l'actinométrie atmosphérique et sur la constitution actinique de l'atmosphère."

Prof. Dr. Gieseler, of Bonn, Germany: "Mittlere Tagestemperaturen von Bonn, 1848-88."

Dr. Ludwig Ilosvay von Nagy Ilsova, Professor in the Royal Joseph Polytechnic School, Budapest, Hungary: "Ueber den unmittelbar oxydirenden Bestandtheil der Luft."

Dr. A. Magelssen, of Christiania, Norway: "Ueber den Zusammenhang und die Verwandschaft der biologischen, meteorologischen, und kosmischen Erscheinungen."

Dr. A. Marcuse, of the Royal Observatory, Berlin, Germany: "Die atmosphärische Luft."

Prof. C. Nees, of the Polytechnic School, Copenhagen, Denmark: "The Use of Kites and Chained Air-balloons for observing the Velocity of Winds, etc."

Surgeon Charles Smart, of Washington: "An Essay on the Properties, Constitution and Impurities of Atmospheric Air, in relation to the promotion of Health and Longevity."

Dr. F. Viault, of the Faculty of Medicine, Bordeaux, France: "Découverte d'une nouvelle et importante propriété physiologique de l'Air atmosphérique (action hématogene de l'air raréfié)."

(Signed), S. P. LANGLEY,
G. BROWN GOODE,
JOHN S. BILLINGS,
M. W. HARRINGTON.

August 9, 1895.

THE PERSEIDS OF 1895.

THE conditions have been very unfavourable for the observation of this meteoric display. The moon's presence in the firmament overpowered the smaller meteors, and unfortunately the weather was very unsettled, the first half of August being notable for its frequent rains and clouded skies.

It was intended to obtain some observations at the end of July before moonlight interfered, but the attempt failed at several stations. On July 25, however, Prof. A. S. Herschel, at Slough, availed himself of a pretty clear interval between 11h. and 12h. 40m. to watch for Aquarids and early Perseids. He found meteors rather bright and plentiful, and the chief radiants in Cassiopeia, Camelopardus, Perseus, Aquarius, and Capricornus. At 11h. 32½m. an Aquarid brighter than Jupiter was recorded in a position a few degrees north of the head of Draco, and at 11h. 55m. a bright Capricornid, equal to Jupiter, traversed a long slow course from the north-east region of Cassiopeia.

On August 2, Mr. E. R. Blakeley, of Dewsbury, watched the sky from 11½h. to 14½h., and observed thirty-one meteors, of which seventeen, or slightly more than one-half, were Perseids with a radiant about 3° in diameter at $35\frac{1}{2}° + 52°$. Mr. Blakeley regards the declination as rather uncertain; it is probably 3° S. of the real position. The brightest meteors seen were Perseids; very fine ones were noted at 13h. 33m. and 13h. 45m.

On August 7, between 10h. and 12½h., some meteors were observed at Slough, Bridgwater, and Bristol. Prof. Herschel at the former place found them very scarce, however, for though the sky was quite clear from 10h. 50m. to 12h. only four meteors were detected. Mr. Corder, at Bridgwater, noted twelve in a watch of 2½ hours. Three or four of the paths indicated a good radiant at η Persei, but others seemed to come from just below γ. At Bristol the writer recorded seven meteors in 1¾h., and of these five were Perseids with a radiant at $41° + 57°$, which agrees with the usual position on August 7. Three meteors were observed at more than one station, and the particulars are as follows:

10h. 12m.—A swift, streak-leaving meteor of 2-3 magnitude observed at Bridgwater and Bristol. Height at beginning 43 miles over Bromyard, Hereford, and it disappeared at an elevation of 28 miles near Crickhowell, Brecon. The real length of path was 42 miles, and the earth-point at Barnstaple, Devon. The radiant was at $45° + 47°$, so that it was not a true Perseid, but a member of a well-known contemporary shower near α Persei.

11h. 4m.—A fine moderately swift meteor variously estimated as first magnitude, equal to α Lyræ, and Jupiter by observers at Bridgwater, Slough and Bristol respectively. Height at beginning 74 miles, at end 45 miles. The meteor passed from above Newport, Mon., to Gellygaer, Glam. Real length of path 33 miles. Earth-point 5 miles north of Pontardawe. Radiant at $333° + 36°$ in the south region of Lacerta.

11h. 29m.—A swift, streak-leaving meteor of second magnitude observed at Bridgwater and Bristol. Height at beginning 105 miles over Stratford-on-Avon, at end 63 miles over Oldbury-on-Severn. Real length of path 64 miles. Earth-point near Chumleigh, Devon. Radiant at $38° + 57°$, so that the meteor was a true Perseid.

On August 9, Mr. Corder, at Bridgwater, watched from 10h. 34m. to 13h. 45m., and saw about 30 meteors, nearly all of which were Perseids. He found the radiant indefinitely marked. A certain proportion of the meteors

observed agreed with a centre at $43° + 57°$, but others were directed from η Persei, and others again from the cluster at χ Persei. On August 10 the writer, at Bristol, watched the eastern sky from 13h. 46m. to 15h. 17m., and saw 19 meteors, of which 17 were Perseids from a well-defined radiant at $45° + 55°$. This is about 2° S. of the correct place. More meteors would have been seen but for the interference of passing clouds.

On August 11, between 10h. and 11h. at Bristol, 11 meteors were observed, including 7 Perseids with radiant at $44° + 58°$. Clouds were again very prevalent, and greatly restricted the view.

On the same night, Prof. Herschel, at Slough, had a clear sky from 9h. 50m. to 12h., and mapped twenty-six meteors, a great majority of them being Perseids. Many of the meteors were bright, and Prof. Herschel regarded the maximum frequency as occurring on this date. "Besides Perseids, a few bright meteors diverged from Pegasus, Pisces, and the head of the Lynx. A pseudo radiant (probably) of the Perseids presented itself at $46° + 63\frac{1}{2}°$. But the body of the Perseid radiation is very scattered—only the tail end of the shower being here recorded very likely—and a large area enclosing γ, τ, η, χ Persei and H, B, C, D Camelopardi, with its centre at about $43° + 58°$, near k Persei, is the best approximation that can be gathered from the tracks registered."

A fourth magnitude meteor, moving swiftly, was seen at 10h. 7m. both at Slough and Bristol. Height at beginning, 78 miles; at end, 62 miles. It passed from over Brackley (Northampton) to Farringdon (Berks). Real length of path, 30 miles; earth-point, 10 miles south-west of Portland, Dorset. The radiant was at $48° + 60°$, the meteor being a true Perseid.

From the various reports already received, it appears certain that this year's display has been far from presenting a conspicuous character. This has probably not proceeded from any special weakness in the shower itself, but from the unsuitable circumstances which have attended its return. Moonlight is a most serious obstacle in the way of meteoric work, and when, added to this, the observer is confronted with skies more or less clouded, the chances of success become very remote. But, in spite of these untoward conditions, the shower has by no means passed unobserved; many of its brilliant meteors have been recorded, and the radiant point has been determined on several nights. Some of the chief contemporary systems have made their presence known by some fine objects, and the results on the whole may be regarded as very satisfactory.

W. F. DENNING.

SIR JOHN TOMES, F.R.S.

ANOTHER of the small band of histologists, who took up the subject when the field was almost untrodden, has passed away, at the age of eighty.

Sir John Tomes, after serving an apprenticeship to a medical man at Evesham, came to London in 1836, and entered at King's College and at the Middlesex Hospital, being at the former a class-mate with the late Sir William Bowman, with whom a life-long friendship thus began.

For two years (1839–40) he resided in the Middlesex Hospital as house-surgeon; and even at this early stage in his career his attention became turned towards the histology of bone and teeth, and we find him feeding a nest of young sparrows and a sucking-pig upon madder. From a somewhat fragmentary diary which he kept, we find, too, that he then bought from Powell (afterwards Powell and Leland) a microscope, and that he was often spending his evenings with Bowman, Quekett, Kiernan, Todd, Carpenter, and Edward Forbes.

He was an early member of the Microscopical Society, and over a long series of years his contributions to the histology of the hard tissues were numerous. Amongst his more important papers in the *Phil. Trans.* were those on bone (in conjunction with the late Campbell de Morgan), on the dental tissues of marsupials, of rodents, and upon the structure of dentine, this last establishing the existence in dentine of the soft fibrils, ever since known as "Tomes' fibrils."

Like that of his friend Bowman, almost all of his work has stood the test of time, and to this day remains undisturbed. A strong bent towards mechanical invention led him, while still house-surgeon, to revolutionise the construction of tooth forceps, which thenceforward supplanted the old "key" instrument; and at the advice of the late Sir Thomas Watson, he determined to devote himself to the practice of dental surgery, in which the busiest years of his life were spent.

Dr. Morton, a dentist of Boston, Mass., having introduced the use of ether in 1846, we find from Sir John's diary that he was early in the field as an experimenter with this anæsthetic. After sundry experiences with it for tooth extraction at the Middlesex Hospital, some successful and some not, we read: "Gave ether to Arnott's case of lithotomy eight minutes, and insensibility came—the operation then commenced and lasted twelve minutes." (Jan. 14, 1847.) And after notes of many administrations: "Gave ether to eight patients for operations with great success. Earl of Cadogan (a governor of the hospital) and many others present." (Feb. 23, 1847.)

His lectures on dental physiology and surgery were perhaps the first in which the subject was treated from a true scientific standpoint, and when published became quite a classic. But it is curious to read in his diary a resolve that he really will not deliver any more lectures unless he has a class of at least six students.

In 1883 the College of Surgeons, exercising their right to confer honorary fellowships of the College, elected Sir John Tomes and the late Prof. Huxley.

In 1886 he obtained the honour of knighthood, in recognition of his great services to the cause of dental education, and to the establishment of a dental diploma and its recognition by Parliament, his unbroken success in all that he undertook being largely due to his excellent business capacity, and to the respect, trust, and liking which he inspired in all with whom he came in contact.

NOTES.

WE understand that a Civil List pension of £200 has been granted to Mrs. Huxley.

THE following have been elected Associates and Correspondents of the Reale Accademia dei Lincei:—National Associates, Prof. L. Luciani and Prof. G. Tizzoni; Correspondents, Prof. E. Cesàro, Prof. A. Ricco, and Prof. Carlo de Stefani; Foreign Associates in Mathematics, Prof. C. Jordan and Dr. G. Salmon; in Astronomy, Prof. Simon Newcomb; in Physics, Prof. H. J. Wild; in Morphology, Prof. A. Kölliker.

The following are among the recent appointments abroad:—Dr. R. Behrend to be Professor of Chemistry in the Technische Hochschule of Hanover; Dr. X. Siefert to be Professor of Forestry at the Technische Hochschule of Karlsruhe; Dr. F. Richarz to be Professor of Physics in the University of Griefswald; Dr. P. Stäckel to be Assistant Professor of Mathematics in Königsberg University; Dr. O. Wiener to be Professor of Physics in the University of Giessen.

REUTER's correspondent at Wellington reports that a severe earthquake shock was felt at Taupo, in the district of Tauranga, and at some other places in New Zealand, on Saturday last. An earthquake was also felt over the greater part of Peru, but principally in the south, on Monday.

WE learn from *Das Wetter* that the efforts which have been made during the last fifteen years for the re-establishment of a

meteorological observatory on the Brocken, have at last been crowned with success, and, if unforeseen difficulties do not arise, it is expected that this important station will be in working order during the coming autumn. This successful issue is mostly owing to the support given to the undertaking by the Ministry of Public Worship and the Meteorological Institute of Berlin, and by the Brunswick and Hanover sections of the German and Austrian Alpine Club. There can be no doubt that observations from this mountain observatory will be of considerable value for the progress of meteorological science.

As already announced in these columns, the sixty-seventh meeting of German physicians and men of science will take place at Lübeck on September 16 to 21. Members and visitors will be received at the Town Hall on Sunday, the 15th, at 8 p.m. Business will commence on Monday at 11 a.m. in the Gymnastic Hall with a presidential address, followed by some medical papers. At 3 p.m. the sections will be formed, and at 7 p.m. there will be a social gathering at the Tivoli. Among the entertainments of the following days, are a garden party given by the Senate of the Free Hansa City of Lübeck on Tuesday, a grand ball in the theatre on Thursday, and an excursion to the lakes of East Holstein on the Saturday. Medical papers are announced by Drs. Klebs, Behring, Riedel, and Rindfleisch, and general scientific papers by Drs. Victor Meyer, Ostwald, and others. Senator Dr. Brehmer and Dr. Theodor Eschenburg are the secretaries of the meeting.

THE *Board of Trade Journal* reports that an industrial exhibition, to celebrate the jubilee of the recognition of Berlin as the capital of the German Empire, is to be held next year in the Treptow Park, near that town, from May to October. The exhibition will embrace the following groups:—(1) Textile industries; (2) Clothing industries; (3) Building and engineering; (4) Wood industries (cabinet-making, &c.); (5) Porcelain, glass and fire-brick industry; (6) Smallwares and fancy goods; (7) Metal industry; (8) Engraving, the decorative arts, and the book trades; (9) Chemical industry; (10) Food products (including tobacco, spirits, &c.); (11) Scientific instruments; (12) Musical instruments; (13) Machine-construction, shipbuilding, and transport trade; (14) Applied electricity; (15) Leather and india-rubber industry; (16) Paper industry; (17) Photography; (18) Hygiene, and sanitary dwellings; (19) Education and instruction; (20) Fishing and boating, as industries and sports; (21) Riding and racing, aquatic sport; cycling, shooting and hunting, pleasure-boating; (22) Horticulture; (23 German colonial exhibition; (24) Hotel and restaurant trades.

THE Council of the Federated Institution of Mining Engineers have had for some time under their consideration the holding of meetings of the student members, and the first meeting of students was successfully held in the North of England district on August 13, 14, and 15. With a view to interest the students more especially in the proceedings of the first meeting, a prize was offered by the Institution for the best essay on "The Prevention of Accidents in Mines." The prize was obtained by Mr. Austin Kirkup, whose essay deals concisely with the commonest forms of mining accidents, and sets forth the results of the experience of practical men on their prevention. Mr. Kirkup has based his facts almost entirely on the knowledge which practical experience and observation have afforded him, so his essay possesses a real value, and we regret that pressure upon our space prevents us from doing more than refer to it. In order that the meeting in connection with which the paper was prepared might be of a thoroughly practical character, the students who took part in the proceedings made lengthy underground visits to the Wearmouth and Eppleton Collieries, and were given every information as to the mode of working, haulage, ventilation, &c., practised at these extensive collieries. The Institution is to be congratulated upon its new departure, which is certainly calculated to give the students a wider knowledge of mining than they would otherwise obtain.

WE have received the official programme of the prizes offered for 1896 by the Société Industrielle de Mulhouse. A prize of 1250 francs is offered for a complete history of one of the principal branches of Alsatian industry, such as spinning and weaving cotton and wool, printing woollen and cotton fabrics, machinery, &c. The Hubner prize, represented by a *médaille d'honneur* and 1000 francs, is offered for the best memoir on the carding of spun textile materials during the period which has elapsed since the last publication on the subject, or for the improvement which, in the opinion of the Society, shall have contributed most to the development of carding operations. Similar prizes are offered for a substance which, in the coloured cloth industry, can replace the dry albumen of eggs, and is cheaper than this substance; and for a colourless blood albumen which does not colour on steaming. Silver medals and prizes of 500 francs each are offered for a new and simple means of determining the amount of priming in steam boilers; for a new and advantageous mode of constructing buildings suitable for cotton and wool spinning and weaving, or the manufacture of dyed cloth; new and practical researches on the movement and cooling of steam in long conduits; a registering pyrometer for steam boiler fires; a memoir on the spinning of carded wool; and for a complete memoir on the drying of tissues. Besides these prizes, medals of various grades are offered in some 140 subjects connected with chemical and mechanical arts, agriculture, commerce, history, and fine arts. The competitions are international, but it does not appear from the programme whether French is to be the only language permitted. The memoirs, designs, samples, &c., must be marked by a device or motto chosen by the author, and addressed to the President of the Society before February 15, 1896, together with a sealed envelope containing the exact name and address of the competitor.

MR. T. H. BICKERTON pointed out, at the recent meeting of the British Medical Association, that when the inquiry was arranged into the disastrous collision between the *Elbe* and the *Crathie*, it was stated that "the question of the powers of vision will be carefully borne in mind in the Board of Trade inquiry into the cause of the collision." The inquiry has now been concluded, but it appears that the witnesses were not examined as to their eyesight. This act of negligence will need a deal of explaining. The reading of Mr. Bickerton's paper was followed by the adoption, on the proposal of Dr. Farquharson, M.P., of a resolution that the matter should at an early date be brought to the notice of Parliament, which should be asked to insist that adequate tests should be compulsorily applied before a lad is apprenticed to the sea; that the Royal Society's recommendations should be acted on by the Board of Trade in their entirety; and that officers already holding certificates, and now by the institution of adequate tests found colour blind, should have shore berths given them in Government offices.

THE morphological place of moulds and yeasts, respectively, has long been the subject of speculation and research, some authorities regarding yeasts as having an independent existence, others considering them as only transitory forms in the life-history of moulds. Most important and interesting contributions to this subject have recently been furnished by the experiments carried on in Dr. Jörgensen's laboratory in Copenhagen. In the course of some researches on the diastatic power of the well-known Japanese mould *Aspergillus oryzæ*, Juhler found that in the flasks in which this mycelium had converted rice-starch into sugar, it had produced a growth of typical alcohol

producing saccharomyces cells. This most interesting observation was subsequently confirmed by Jörgensen, who has since endeavoured to ascertain if the various types of alcohol producing yeasts can be traced to particular moulds, and already he has succeeded in demonstrating the evolution of wine yeast cells from a particular mould extensively present on grapes. Dr. Jörgensen intends to continue these most suggestive investigations, and publish his results from time to time in the form of a separate *Bericht* exclusively devoted to the work carried out in his laboratory. In pursuing these researches Dr. Jörgensen will not only render great practical service to the science of fermentation, but he will also lay botanists under deep obligation to him for having rendered possible a more extended and accurate insight into the life-history of moulds.

THE annual address on "The Recent Evolution of Surgery," delivered before the Medical Society of London in May last, by Mr. A. Pearce Gould, has been published in the form of a dainty brochure by Messrs. Kegan Paul and Co.

THE *Transactions* have reached us of the Ballarat meeting (1894) of the Australasian Institute of Mining Engineers. Among the papers contained therein, we notice a review of past and present steam pumping in mines, by Mr. J. Tipping; an address on the mineral wealth of Victoria, by Mr. James Stirling; an account of the physiography and geology of the Wadnaminga Gold Field, by Mr. F. D. Johnson; notes on the White Cliffs Offal Fields, Wilcannia, by Mr. F. G. de V. Gipps; and a description of ore-dressing by automatic machinery, by Mr. H. W. F. Kayser.

WE have received from Dr. G. Hellmann, of Berlin, a revised edition of "Meteorologische Volksbücher," which first appeared in *Himmel und Erde* in 1891 (see NATURE, vol. xliv. p. 185). The work contains an account of the earliest popular German treatises on natural science and meteorology from the first encyclopedia, "Lucidarius," which was written more than two centuries before the invention of printing, to the "Hundred-year Calendar" of Dr. Knauer, for the years 1701-1801. Dr. Hellmann has embellished the work by further biographical notes and additions based upon his laborious researches since the appearance of the first edition.

THE forty-first annual report of the Trustees of the Australian Museum is not a pleasing one. We read: "The continued smallness of the income allowed to the Trustees by Parliament has practically stopped the acquisition of specimens by purchase or collection. The amount expended in the purchase of specimens [during 1894] does not exceed £20. No collecting expeditions have been sent out, all that has been done in this way being confined to flying trips around Sydney. . . . The staff still continues at its reduced strength, and the forced economies of late years are beginning to tell on the efficiency of the Institution." It is really time that something was done to alter this unsatisfactory state of things; for the present conditions hamper the usefulness of the museum, and are most detrimental to the interests of science. A few researches have been carried on by the officers of the museum, and the mention of them gives a little light to an otherwise rather discouraging report.

MESSRS. GEORGE PHILIP AND SON have published a school edition of the "Systematic Atlas." The atlas has been specially arranged for the use of students in higher schools and training colleges. Though an abridgement of the "Systematic Atlas," it contains as many as 170 maps—practically all the general ones—in forty-one plates, and a complete index of more than 12,000 names. The atlas will be very valuable for class work in physical and political geography, and is a useful introduction to the larger edition, which has already been reviewed in these columns.

Another atlas, of which Messrs. Philip have just published a new edition, is the "Handy-Volume Atlas of the World," by Mr. E. G. Ravenstein. This, however, is almost a new work, for the whole of the maps have been re-drawn and re-engraved, and the letterpress accompanying them has been rewritten. We reviewed the original edition when it appeared some years ago; and it is only necessary now to say that the present volume, like its predecessor, is a compact and an efficient pocket-atlas.

THE second part of the fifty-first volume of the *Verhandlungen des Naturhistorischen Vereins der Preussischen Rheinlande, Westfalens und der Reg. Bezirks Osnabrück* (Bonn, 1894), contains six memoirs and a series of shorter papers and notes. The first memoir is a list of the fossils derived from northern regions found in the diluvial deposits of Westphalia, which is contributed by Dr. W. von der Marck. Laspreyes has issued a detailed study of the meteorites in the museum of the University of Bonn, in which the literature is tabulated with great care. Stockfleth describes the iron-ore deposits in the Hill of Huggel, near Osnabruck, where it occurs in the Zechstein. C. Roettgen gives a "Contribution to the Coleoptera Fauna of the Rhine Province." H. Pohlig continues his study of abnormal deer antlers by a description of two pairs belonging to the great Irish elk. One of these has a brow tyne on the left side, but no trace of one on the right, whereas the first of the serial tynes on that side is branched. In the other case both brow tynes are present, but the second serial tyne on the left side has a rudimentary branch. Dr. Verhöff contributes a short paper on the biology of the fire-fly *Phosphænus hemipterus*. Among the smaller papers, a note by Ludwig gives a brief account of Marchiafava and Celli's work on the malaria parasite; Philippson summarises the geological problems that still await solution in Western Turkey. Schenck gives a brief demonstration of the structure of the Brazilian lianas, or climbing stems.

THE additions to the Zoological Society's Gardens during the past week include a Ruffed Lemur (*Lemur varius*, ♀) from Madagascar, presented by Mr. J. H. Bingham; a Vervet Monkey (*Cercopithecus lalandii*, ♀) from South Africa, presented by Mrs. C. J. Humphrey; a Mozambique Monkey (*Cercopithecus pygerythrus*, ♀) from East Africa, presented by Mrs. John Mahon; a Sooty Mangabey (*Cercocebus fuliginosus*, ♀) from West Africa, presented by Mr. Davies; a Sykes's Monkey (*Cercopithecus albigularis*, ♂) from East Africa, presented by Mr. J. Watkinson Brown; a Cheetah (*Cynælurus jubatus*), a Blotched Genet (*Genetta tigrina*) from Somaliland, presented by Mr. J. L. Harrington; a Martial Hawk Eagle (*Spizaetus bellicosus*) from British East Africa, presented by Captain B. L. Sclater; two Ravens (*Corvus corax*), two Buzzards (*Buteo vulgaris*), two Greater Black-backed Gulls (*Larus marinus*), European, presented by the Hon. William Edwards; a Herring Gull (*Larus argentatus*), British, presented by Mr. George Hawes; two Orbicular Horned Lizards (*Phrynosoma orbiculare*) from Mexico, presented by Mr. Bernard Jackson; a Rhesus Monkey (*Macacus rhesus*, ♀) from India, a Black-backed Jackal (*Canis mesomelas*) from South Africa, four Spiny-tailed Mastigures (*Uromastix acanthinurus*) from North Africa, deposited; two —— Octodons (*Ctenodactylus gundi*) from Egypt, purchased, three Dorcas Gazelles (*Gazella dorcas*, ♀ ♀ ♀) a Sœmmerring's Gazelle (*Gazella sœmmerringi*, ♂), an Egyptian Cat (*Felis chaus*), three Libyan Zorillas (*Ictonyx lybica*), ten Varied Field Rats (*Isomys variegatus*), thirty-five Hairy-footed Jerboas (*Dipus hirtipes*), forty-five Lesser Egyptian Gerbilles (*Gerbillus ægyptius*), eight Larger Egyptian Gerbilles (*Gerbillus pyramidum*), two Egyptian Kites (*Milvus ægyptius*), a Cerastes Viper (*Vipera cerastes*) from Egypt, received in exchange; a Spotted Pigeon (*Columba maculosa*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE CŒLOSTAT.—The name *cœlostat* has been given by M. G. Lippmann to a modified form of siderostat which he has devised (*Comptes rendus*, No. 19, 1895, and *Observatory*, August). The special feature of the instrument is that it gets rid of the rotation of the field of view which disqualifies the siderostat for some purposes, such, for instance, as long-exposure photography. It consists simply of a mirror with its plane parallel to the earth's axis, and turning on a polar axis once in forty-eight hours in the same direction as the apparent diurnal motion of the heavens. It is easily demonstrated that the image of any star whatever will be seen stationary in a mirror so mounted, and a telescope pointed at the mirror in any direction will have a constant field of view. The telescope being directed to the cœlostat in a given position, to observe other objects having the same declination as that in view, it will only be necessary to turn the mirror; but for objects with different declinations the telescope must also be moved. If it be desired to use a horizontal telescope, it must be directed to the point on the horizon where the object rises, and the mirror must be started in a position suited to the hour-angle; but there is a limit to the use of a horizontal telescope. It is pointed out that the simplicity of the instrument makes it possible to turn it into one of great precision; stability being readily attained, while the possibility of flexure can be reduced to a minimum.

ADAMS' MASSES OF JUPITER'S SATELLITES.—A question having been recently raised by Mr. Marth as to the work of Adams on Jupiter's satellites, Prof. R. A. Sampson has stated the results of an inspection of the MSS. with reference to this subject (*Observatory*, August). It appears that when engaged upon a revision of Damoiseau's tables in 1875, with a view to their continuation, Prof. Adams determined the following revised values for the masses of the satellites:—

$$m = 0{\cdot}0000283113$$
$$m' = 0{\cdot}0000232355$$
$$m'' = 0{\cdot}0000812453$$
$$m''' = 0{\cdot}0000214880$$

"There is no reason to suppose that Adams attached any weight to the above determinations of the masses, seeing that he never published the values directly; the MS. appears to be little more than a study such as he was in the habit of making upon any work that he was examining, in order to test by cross verifications the accuracy and consistency of the whole. . . . Considerable expectations have been built upon the fact that Adams was engaged more or less closely for some years upon the theory of Jupiter's satellites. It will be well to say at once that the chief fruit of his attention was published in the *Nautical Almanac* of 1880; this, like all the rest of his published work, was the result of exhaustive labour, quite out of relation to the unpretentious form in which the outcome was presented, and only discoverable by searching tests."

ATMOSPHERIC REFRACTION.—The ordinary application of Bessel's expression for refraction requires that five quantities be taken from specially prepared tables, but Prof. E. C. Comstock, Director of the Washburn Observatory, has worked out a simple formula for computing the refraction without the aid of tables. A transformation of Bessel's formula, and the introduction of numerical constants from the Pulkowa refraction tables, leads to the following simplified form:

$$R = [2{\cdot}99215] \frac{BF}{455{\cdot}9 + t} \tan Z$$

$$\log F = -(42{\cdot}3 + 0{\cdot}12t) \tan^2 Z.$$

The number in brackets is a logarithm; B is the barometric pressure in English inches reduced to freezing-point; t is the temperature in degrees Fahrenheit, and Z is the zenith distance for which the refraction is required. The formula for F gives the logarithm in units of the fifth decimal place.

The computation by the formula is not more laborious than the direct use of the tables, and a comparison of the two methods shows that the differences in the results are far less than the uncertainty in the tabular numbers themselves. Prof. Comstock's paper forms one of a series of interesting "Studies in Spherical and Practical Astronomy," in the *Bulletin* of the University of Wisconsin (vol. i. No. 3).

ON THE ORIGIN OF EUROPEAN AND NORTH AMERICAN ANTS.

QUESTIONS belonging to zoogeography may be practical or theoretical, actual or genetic; ultimately the resolution of them, whatever they may be, takes its chief interest from their relations to genetical problems, that is, to the explanation of the origin of actual faunæ, and to the knowledge of the original home of phyletic groups, and of the ways followed in their gradual diffusion over the whole or part of the world. To this purpose, not only living animals, but also fossils, have to be determined, and their affinities exactly worked out; changes in the distribution of land and sea and in the shape of continental areas must be investigated, and analogies and differences in the diffusion of various groups of living beings taken in consideration, as far as they are known. The work involved is long and difficult, and its results will form the science of the future.

In a paper published in 1891, on the fossil ants of Sicilian amber,[1] I made out that at the beginning of the Miocene epoch, North and South Europe had very different faunæ of ants, the Sicilian amber containing genera which belong to the actual Indian and Australian fauna, but wanting the typical holarctic genera *Formica*, *Lasius*, *Myrmica*, which are found in the Baltic amber, some species of them being extremely common and abundant. A similar, but not such a striking, difference exists between recent Mediterranean and North European ants, the former including a greater percentage of Indian and cosmopolite forms, and an absolutely and relatively lesser number of typically holarctic ones, the most species of *Formica*, *Myrmica*, and *Lasius* not having reached Africa (*F. fusca*, L., and *M. scabrinodis*, Nyl., are introduced in gardens in Algeria), and these genera being scarcely represented in Mediterranean islands. After discussing these facts, I came to the conclusion that South Europe should have had in the Tertiary epoch an ant fauna compound of old Mesozoic cosmopolite genera (chiefly Ponerinæ), mixed with Indian-Australian forms. In North Europe these lived together with northern genera, which, after the emergence of the bottom of the middle European sea, invaded the South, being perhaps expelled from the North by gradual cooling of climate. Later, the glacial epoch destroyed in Europe nearly all the rest of tropical insects, their return being made impossible by the natural barriers of sea, deserts, and mountains, accumulated southward and eastward of our continent.

These studies I have carried a step further in a revision, now printed, of the Formicidæ of North America.[2] A great number of North American ants are specifically identical to European ones. My attention was directed to find differences between American and European specimens, and indeed but a few species were so similar to their European relatives as to be not distinguishable as sub-species or varieties. The one genus, *Epœcus* and two sub-genera are exclusively Nearctic; all the other genera of North American ants not represented in Eurasia (*Discothyrea* has two species only, one in North America, another in New Zealand) are Neotropical. The northern regions of Europe has the one peculiar genus *Anergates*, allied to *Epœcus*; middle and south Europe have two further genera not found in other parts of the world, and some others known from the Indian region. All these facts lead to the result, that the Palæarctic ant-fauna is made of cosmopolite + Arctic + Indian elements; that the Nearctic fauna is similarly composed of cosmopolite + Arctic + Neotropical ones.

The question that now arises is: how has such a mixture been effectuated—what changes have determined it? A complete and detailed answer I believe to be at present impossible; but the knowledge of the fossil mammals may help us greatly, supplying for the want of evidence taken from fossil ants, other than the Miocene fauna of European amber, the fossil prints of Formicidæ being too imperfectly known, and a careful revision of the existing collections from a trained specialist wanted. I believe that mammals and ants are both of the same age; their migrations took place by means of the same land connections, with the difference, that winged females of ants could, easier than terrestrial mammals, pass over sea-arms, being carried by winds.

I admit that in the Oligocene epoch, after Australia, Africa and South America had been cut off from a great northern

[1] C. Emery. "Le Formiche dell' Ambra Siciliana nel Museo Mineralogico della R. Università di Bologna." (*Memor. Accad. Bologna* [5], vol. v. 1. 1891).

[2] C. Emery. "Beiträge zur Kenntniss der Nordamerikanischen Ameisenfauna. (*Zoolog. Jahrbücher.* Abth. f. Syst. 7 Bd. pp. 633-682, Taf. 22; 8 Bd. pp. 257-360, Taf. 8. 1893-95.)

system of dry land (such a system was rather an extensive archipelago than a continuous continent); this last was again divided into two systems: an Arctic and Occidental one, comprising North America, together with the northern parts of Asia and Europe, and an Indian one, communicating with South Europe. The former was the home of the Cervidæ, the rhinoceroses and most other Perissodactyls, the latter that of the Cavicorns and elephants. Very few mammals of Indian origin migrated into America; much more from the Arctic system into India. The same seems to be the case for ants. *Myrmecina* is perhaps the only North American genus of Indian origin (*Tetramorium cæspitum* being doubtless introduced by man), whereas a number of American-Arctic genera, sub-genera and species-groups, as *Myrmecocystus*, *Messor*, *Myrmica*, *Camponotus pennsylvanicus*, &c., are more or less far diffused in India and Africa, *Myrmica* reaching Borneo, and *Messor* the Cape of Good Hope.

In Europe, the study of the Baltic and Sicilian amber proves that the Arctic fauna went down from the north, as a host of conquerors, invading the territory formerly occupied by other people. I believe that, in Miocene times the North American fauna was much like the actual cosmopolite and Arctic part of the recent fauna, and might have included a number of forms actually extinct. As in the Pliocene a bridge was put between North and South America, an invasion of neotropical forms took place, walking from south to north. But it is not improbable that other forms migrated in the opposite sense, and descended from North America into the neotropical region. I suppose that such was the case for the genus *Pogonomyrmex*, perhaps also for *Dorymyrmex*, *Forelius*, and several species of *Camponotus*. It is not improbable that other genera from North America migrated southward, and later became extinct in their primitive home. The recent work of Mr. Scudder on Tertiary Curculionidæ of North America seems to confirm this view, some of these fossil beetles belonging to genera now living only in South America. It is probable that a number of insects, actually regarded as typical members of the neotropical fauna, immigrated from North America, as it is proved by palæontology for several mammals, as, for instance, the llama and alpacca of the Pampas.

The North American origin of some South American ants was suggested by Prof. H. von Jhering,[1] in a paper published last year. The author endeavours to sustain, by the study of the ants, his theory of the multiple origin of actual neotropical fauna. I agree in many points with him,[2] but I must recognise that the Formicidæ afford but little evidence in favour of his views. Actually, the ants of South America are distributed chiefly in relation to the climate and vegetation, no strong obstacles being put to the wide dissemination of the species, some of which range from Central America or from Mexico to Paraguay and Rio Grande do Sul. Chili is, however, an isolated country, which we may call "a continental island," although it is not surrounded by water. If we should take the Chilian fauna as a standard for the primitive fauna of v. Jhering's Archiplata, that should have been a very poor one, like the fauna of New Zealand, with which it offers a striking resemblance. The most characteristic feature of the Chilian ant fauna is the occurrence of peculiar species of *Monomorium*, like those inhabiting Australia and New Zealand, and of the genus *Melophorus*, found only in Australia and New Zealand. These facts corroborate the hypothesis of a Cretaceous or Eocene connection between South America and Australia.

New Zealand appears as a bit of old Australia, quite free from later Papuan or Indian intrusions, like Madagascar, which, as an isolated part of old Africa, has received but a few immigrants, when, at the Pliocene epoch, a stream of Indian life entered into the Æthiopian continent. Probably Chili may be considered as a part of ancient Archiplata, secured from Guyanean and Brazilian immigrants by the heights of the Cordillera, but having preserved only an incomplete set of the original Archiplatean fauna.

I state these facts for the purpose of making the main conclusions of a special work known to a wide public. Exact knowledge of the exotic faunæ, and especially of the fossils, may enable us in future to carry further these incomplete and in part hypothetical results. Similar studies made on single groups of animals and plants by specialists, which do not only accumulate by blind statistical work names of families, genera, and species, but deal with them, knowing the value of each, are highly desirable. Summarising and integrating the single results will build up an exact knowledge of palæogeography, and of the origins and interrelations of the faunæ and floræ of the world.

C. EMERY.

[1] H. von Jhering. "Die Ameisen von Rio Grande do Sul." (*Berliner entomolog. Zeit.* 39 Bd. 1894. Pp. 321—446. 1894.)

[2] Other points of v. Jhering's theories, which I cannot accept, refer chiefly to the origin and antiquity of island faunæ. In these points I think that Wallace's views are right.

A NEW FILM HOLDER.

NO outdoor photographer can take a rough survey of the past few years without feeling some astonishment at the rapid progress made in nearly every branch of his art. The amateur is no doubt indirectly responsible for much of this advance; for it is through him that other brains have been set to work to satisfy all his many and various wants, in the way of instruments and accessories, to lighten his task at every step.

The camera, which a few years back was a heavy, clumsy and awkward instrument, is now of a light and handy construction, capable of being used in many cases without the tripod. Stops are now more generally of the Iris type, thus eliminating all possibilities of loss or of leaving them behind; while plate-holders are now supplied capable of holding a dozen or more plates, and necessitating the use of only one dark shutter.

The introduction of the film has brought us, however, into a new era; but the full benefit of this improvement can only be best appreciated by those who make use of their cameras while travelling.

Hitherto it has been impossible to make satisfactory use of the enormous advantages of celluloid flat films over glass plates; but now we have before us a holder which seems to give satisfaction, and which should prove a boon to photographers in general.

A holder to be really efficient should be readily adaptable to any ordinary camera; it must contain a large quantity of films,

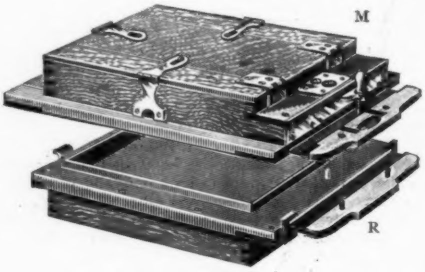


FIG. 1.—Magazine and receiver, separated.

and when complete and loaded should not be any larger or heavier than the three double backs (lighter if possible); and, finally, should be provided with some means of swiftly and automatically changing the positions of the exposed films.

Such a holder, if simple and of moderate price, would be much sought after by the photographic world. A very near approach to such an ideal film-holder will be found in that known as the "Frena," of which a short description follows.

Fig. 1 gives a complete view of the holder (the two parts are here shown separately), ready to be fitted to any camera. It consists of two parts: the magazine (M) and the receiver (R), each of these parts being about half as thick again as an ordinary dark slide. The exposure is made in precisely the same way as with an ordinary dark slide, namely, by inserting the magazine in the slide rails of the camera, and by withdrawing, and subsequently replacing, the shutter of the magazine.

The film changing is brought about simply by folding the magazine and receiver together until they interlock, drawing out the two shutters, pressing a change button to one side, and pushing the shutters back again.

The exposed films, stored in the receiver, may then be removed for development one by one, or as a complete pack, just as the operator desires.

An automatic counter upon the back of the magazine shows at a glance how many pictures have been taken.

The peculiarity of these films is that their edges are notched, and in their packing an alternate sequence is maintained as regards the position of these notches.

The films are supplied ready packed and arranged in the order in which they are to be inserted into the magazine.

To understand more clearly the position of the notches, it is best to take the empty magazine in hand, and entirely withdraw the black exposing shutter. It will then be seen that the front of the magazine is provided along its sides with two series of projecting teeth; it is upon these teeth that the films inserted into the holder are supported. At one end of the magazine, which we shall call the top, is a button; if this button be pushed from one side to the other, this movement will shift all the sorting teeth at the same time, so that they will occupy positions a little to one side of their former ones.

A film introduced into the magazine will then be supported by the sorting teeth, when these stand in the original positions; if this film be put into the holder with its notched corners towards the top end of the magazine. It will, however, fall past the sorting teeth, which pass through its notches, when the change button is moved to one side and the sorting teeth stand in the second position mentioned.

The process of filling the magazine is very simple, for the pressure-board has only to be removed, and the films inserted nto the holder with the white film downwards, *i.e.* towards the pointers, and eventually towards the lens. Should there remain in the magazine any unexposed films, with their backings, and it is merely required to add to their number, the additional films with their backings may be dropped into the holder by twos or threes, due care being taken that the alternate arrangement be maintained.

The whole process, although somewhat lengthy to describe, is in itself very simple and neat, and can be at once grasped by an examination of the holder itself in daylight.

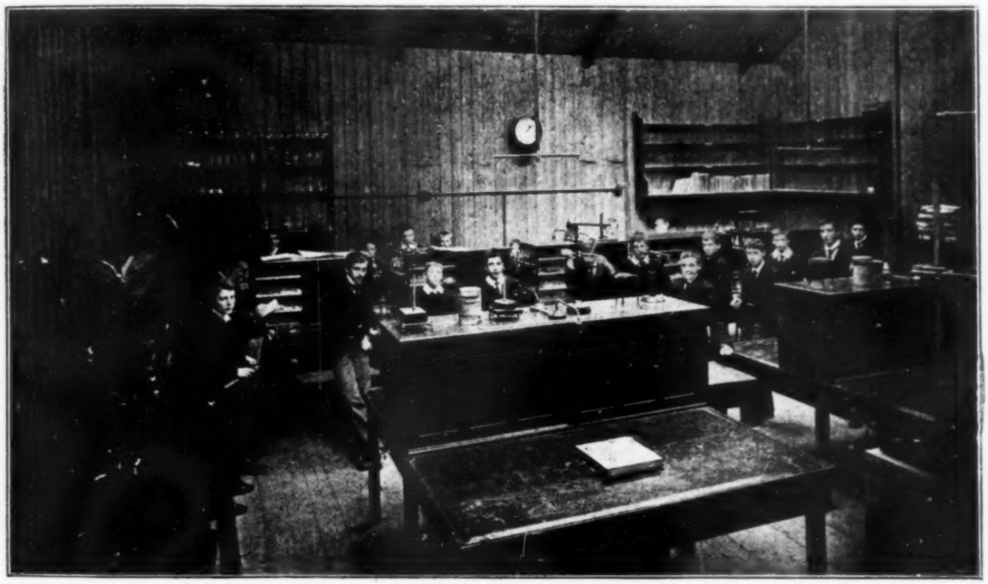
THE NEW NATURAL SCIENCE SCHOOLS AT RUGBY.

THIS new building for the physical part of natural science, which has recently been opened at Rugby School, is well worth a visit from any one engaged in teaching that subject. The building, owing to want of funds, is not at present of a permanent nature, being of the felt and matchboarding type, and in consequence has no pretensions to structural beauty; but when funds are forthcoming, no doubt the whole will be built in brick, and this will enable any alteration or improvement which may then be deemed necessary to be made. The building comprises a lecture room, a large laboratory, two small laboratories, an apparatus room, and workshop. At the back is another large room to be used for a natural history museum.

Every room is fitted with electric light and Ridge ventilation, which keeps the air pure even when filled with workers. The lecture theatre, which is capable of holding from 80 to 100 boys, is fitted with a solid slate table on brick piers, so that work can be done on it with the most delicate instruments without interference from the vibration of the floors. The fact that the rooms are all on the ground floor, gives the opportunity of putting all delicate instruments, such as balances, galvanometers, &c., on brick pillars, and thus to get rid of any vibration whatsoever.

The main laboratory contains ten tables for elementary physical measurements, two for calorimetry, two for magnetism, and two for heat experiments. Each table has a cupboard containing the necessary apparatus, and an electric lamp giving direct illumination on the tables without shadow or glare in the eyes of the worker.

Of the two smaller laboratories, one is an optical room, which can, of course, be completely darkened, and is fitted with two optical tables and a heliostat, so as to use direct sunlight as often as possible.



The other is the electricity room, containing two tables for frictional, and two for voltaic electricity, with cupboards, &c., as in the main laboratory.

Provision has been made for a small engine and dynamo for electrical work, and these will no doubt be added in time.

The whole is under the charge of Mr. L. Cumming, to whom the arrangement is due, and who is certainly to be congratulated on the result.

Every boy who takes up natural science at Rugby not only goes through a course of lectures, but has also to do experimental work himself in the laboratory. This enables him to grasp the subject much more thoroughly, and to remember it much better than if he attended the lectures only. That this method has had excellent results, will be seen by the number of successes in natural science that Rugby has gained of late years in scholarship and other examinations.

EVIDENCE OF A TWILIGHT ARC UPON THE PLANET MARS.

DURING last summer and autumn Mr. Douglass made at this observatory 341 micrometric measures of the diameters of Mars. In addition to their general value as micrometric measurements, these turn out to be of a peculiarly interesting

character. For on reducing them I find that beside furnishing, from their great number, relatively accurate values of the equatorial and polar diameters and of the polar flattening, they yield a by-product as unexpected as it is important. Their discussion reveals, in short, what appears to be unmistakable evidence of a twilight upon the planet, sufficiently pronounced to be visible from the earth, and actually to have been measured unconsciously by Mr. Douglass. That Mars possessed an atmosphere, we had what amounted to proof positive before; but that the fact should again be brought to light in this literal manner, as a silver lining to a cloud of figures, is a point of some curiosity. The measures had no such end in view; indeed, to detect the presence of an atmosphere by measures of the diameters had not suggested itself to any of the most adventurous of observers. Yet, as will be seen, the quantities upon which the evidence rests are so large as to be quite without the pale of accidental error, being ten times as great as the probable errors of observation, and twice as large as those that disclose the polar flattening. That they have hitherto escaped detection is due to their having been masked by another factor affecting the size of the polar diameter, as will appear in the course of this paper. To the unsuspected presence of these two causes, at times nearly offsetting each other, so far as relative values go, is attributable in all probability much of the discrepancy in the determinations of the polar flattening hitherto made.

The first measures were made on July 6, and the last on November 21, 1894. From October 12 they were taken nearly every night. Those here given were all made by Mr. Douglass. Later in the paper I shall introduce others by Prof. W. H. Pickering, which confirm the result. But here at the outset it may be well to point out that whether the results of many observers are to be preferred to those of one is, omitting discourteous personalities, a question entirely of what is to be determined. If the determination be one of absolute quantity, the more observers the better, provided they be good; but if, on the other hand, the determination be of relative magnitudes, one observer is better than many, as his personal equation obligingly eliminates itself, whereas two such equations can by no possibility, short of chance, eliminate each other. Now, in the present case, while the determination of the planet's size, and even to some extent of its polar flattening, are matters of absolute quantity, the evidence of a twilight upon it is one which rests upon relative results. The former, therefore, are subject to any systematic errors there may be; the latter, essentially free of them. In consequence, the by-product in this case is actually more trustworthy than the main results themselves.

Much care was taken in the matter of the Martian measures. In the ones I shall first discuss, those made from October 12 and November 21, Mr. Douglass adjusted the longitudinal thread of the micrometer, parallel or perpendicular, as the case might be, to the planet's polar axis, according to Marth's ephemeris, and then placed himself, so that the line joining his eyes was kept parallel to this thread or to the fixed transverse thread at right angles to it, during any one set of observations, the position being then recorded. As measures were taken in both positions for each diameter at various times, we have here a comparison of some eventual value. In eye-estimates such orientation in the position of the observer is absolutely essential in order to correct his possible astigmatism. Into measures, however, astigmatism enters only to cancel out. For if we consider the matter, it is at once evident that the whole field is distorted in the same proportion, the space between one turn of the micrometer and the next being reduced or expanded in the same ratio as the part of the image measured. The astigmatism thus eliminates itself.

From October 12 to November 21, Mr. Douglass made in all 275 measures; 140 of the equatorial, and 135 of the polar diameter. In the reduction of the measures, account has been taken of the place upon the micrometer screw at which the measures were made, and its appropriate value introduced. For by the forethought of Mr. Douglass in suspecting the possibility of variation, we measured the value of a micrometer turn at different points of the scale to confirm his conjecture.

Preliminary to the discussion of the results, it will be well to explain the corrections determined and applied. The first correction is that arising from refraction. This is the correction due to the differential effect of refraction upon the planet's opposite limbs at the extremities of the particular diameter measured. It depends both upon the altitude of the planet at the time of observation, and upon the inclination at that moment, of the particular diameter to the vertical. In many cases it was so small as not to make itself perceptible in the column.

The correction for aberration, similarly a differential effect, was so utterly insignificant throughout as not to appear at all.

The next correction is that due to irradiation. Toward its determination two different tests were made, in each case upon both Prof. W. H. Pickering and myself; in the one the effect should have been less than in the case of Mars, in the other greater. As in both cases the observers substantially agreed, the results may be accepted as having some impersonal value.

The first test was made upon a railroad switch-head, a white circular disc with a smaller black circle painted upon it. The size of these circles was unknown to the observers.

Their estimates were:

(W. H. P.) ... (white rim) ... 1; (diameter black circle) ... 1·3
(P. L.) ... ... „ „ ... 1; „ „ „ 1″·265

The discs and their distance were then measured and gave:

For diameter black circle ...	...	202 mm.
For radius white rim ...	...	126 mm.
For ratio	...	$1\frac{1}{18}$
For distance from eye ...	...	57 yds.

Therefore 1 mm. equalled 3″·9.

For the amount of the irradiation in seconds of arc, x, assume the amount of the irradiation of the white rim against the general background of earth of a brown colour to have been two-thirds that of the rim against the black circle. We have then, for the first observer, the following equation to determine x.

$$\frac{252 \text{ mm. } 10/3\, x}{212 \text{ mm. } 6/3\, x} = \frac{2\cdot 0}{1\cdot 3}; \text{ from which } x = 9\cdot 2 \text{ mm. or } 36'';$$

for the second observer: $x = 40''$.

The second test was on the moon (November 22), when the old moon was seen in the new moon's arms. In this case the irradiation proved for both observers to be one-seventh of the radius of the old moon, or about 157″.

In the case of Mars, the value for the irradiation probably lies between these two limits. For the contrast between the Martian limb and the sky is pretty certainly greater than that of the white rim and the black circle of the switch-head, and less than that of the moon's bright limb and the sky, to which the contrast between the limbs of the old and of the new moon closely approximates.

It is to be noted that with a given illumination and a given eye, the irradiation correction is a personal constant, not depending upon the size of the disc measured and diminishing inversely as the magnification. In all the measures subsequent to and including October 15, the power used was 860; in those of October 12, it was 617. The correction, therefore, for all except those of October 12 was 0″·10; for those of October 12, 0″·14.

Such, then, is the correction for irradiation upon the planet's limb. The double of it, therefore, would need to be subtracted from the measures of a disc similarly placed to that of Mars when fully illuminated. But the disc of Mars was not fully illuminated even at the moment of opposition, and grew less so as time went on. Now it will be evident on consideration that the irradiation from the terminator must be very different from that upon the limb, inasmuch as the light fades away to nothing at the one, while it has its full value at the other.

To determine the amount of the correction needed at the terminator it is to be observed that if

γ = the areocentric angle between the sun and the earth;
α = the angle between the terminator and the point of the illuminated surface of which the irradiation is sought; and
m = the ratio of the irradiation at the limb to the radius of the disc, we have for the extent of the irradiation at the terminator

$$m\left(\frac{\sin \alpha}{\sin (\gamma+\alpha)}\right)^{\frac{1}{n}} - \left(\cos \gamma - \cos (\gamma+\alpha)\right)$$

where n denotes the ratio of the irradiation to the illumination, and is equal to about 2^6; that is, it takes 2^6 times the illumination to produce twice the irradiation effect. This value is got from inter-comparison of the above tests as limiting values, the

resulting value for Mars and the known decrease in illumination due to the telescopic magnification employed.

To deduce the resulting irradiation we must find the value of which renders the above equation a maximum, and then substitute this value in the equation. To do so directly leads to an equation of so high an order that approximation will be found the better, if indeed it be not the only, method of solution. By this means it appears that the necessary correction does not become insensible, to three places of decimals, till the phase angle, γ, somewhat exceeds 30°.

The formula must be used within the limits for which $\frac{\sin\alpha}{\sin(\gamma+\alpha)} = 1$; beyond them $\frac{\sin\alpha}{\sin(\gamma+\alpha)}$ must be taken as unity.

If the reflection from the disc followed the law of the cosines—that is, if the apparent illumination were always equal to the true one—we should have

$$m\left(\sin\alpha\right)^{\frac{1}{n}} - \cos\gamma - \cos\left((\alpha+\gamma)\right)$$

where α, γ, and n have their previous values, and m = a constant to be determined from the equation, from the value at the limb.

But although this is the formula for the case of a theoretical rough bare globe, it manifestly does not hold in the case of Mars, of which the limbs are not only as bright as the centre of the disc, but much brighter. The previous formula is, therefore, to be preferred to it, although even that formula makes the irradiation correction at the terminator too great as compared with that at the limb.

But it is to be specially noticed that no law of correction for irradiation at the terminator, however big it make that correction to be, is able to do away with the outstanding differences, presently to be noted, of the equatorial diameter at different times upon which the evidence of the twilight arc is based.

There is also the correction for phase. Inasmuch as the phase axis and the polar axis did not in general coincide, there entered into its determination beside the amount of the lacking lune, the angle of inclination of the two axes. So that the amount of the defalcation had to be calculated in accordance for each night. These corrections and their results reduced to distance unity have been calculated and tabulated.

Besides the above there is a fifth correction needed to reduce the diameter measured for the polar one, to the true polar diameter. The diameter measured perpendicular to this, or the apparent equatorial diameter, although not in fact an equatorial diameter, was always exactly equivalent to one, since its extremities were always each 90° distant from the pole. The other, however, was the diameter of the ellipse made by the plane passing through the polar axis, which was inclined to the polar axis by the angle of tilt, and needed, therefore, to be reduced to that ellipse's minor axis. This correction is best applied to the means, and appears in the subjoined table.

Polar Diameters.

	Cor.. measures.	Cor. for inclination.	Further cor. for twilight band.
Oct. 15 to 23 inc.	9″·385	9″·379	9″·356
,, 15 to 1 of 24 ,,	9″·377	9″·371	9″·348
,, 15 to 24 ,,	9″·368	9″·362	9″·339
,, 15 to 29 ,,	9″·375	9″·369	9″·346
,, 12 to 30 ,,	9″·384	9″·378	9″·354
Nov. 2 to 21 ,,	9″·397	9″·390	9″·353

Equatorial Diameters.

Oct. 15 to 23 inc.	9″·420	—	9″·404
,, 15 to 1 of 24 ,,	9″·428	—	9″·402
,, 15 to 24 ,,	9″·424	—	9″·395
,, 12 to 30 ,,	9″·440	—	9″·396
Nov. 2 to 21 ,,	9″·545	—	9″·402

Twilight arc ... ... 10°
Polar flattening .. 1/191 of the equatorial diameter.

As previously explained, no correction is needed for astigmatism, as the measures themselves correct it.

So soon as the measures had been corrected and reduced to distance unity, two things became apparent, both so large as to be almost unmistakable before taking the means. The first was the polar flattening; the other an equally systematic difference in the size of the equatorial diameter according as the measures were made in October or in November. The November measures came out much larger than the October ones; while the corresponding polar measures, on the other hand, showed no corresponding increase. Struck by this fact, and suspecting its cause, instead of taking the mean of all the measures for each diameter, I divided them into sets according to their proximity in date to the time of opposition, and took the mean of these sets.

The means are as follows:—

Polar Diameter.

Mean October 15 to October 23, both dates inc.	9″·379
,, ,, 12 ,, 30, ,, ,,	9″·378
,, Nov. 2 to Nov. 21, ,, ,,	9″·390

Equatorial Diameter.

Mean October 15 to October 23, both dates inc.	9″·420
,, ,, 12 ,, 30 ,, ,,	9″·440
,, Nov. 2 to Nov. 21, ,, ,,	9″·545

Opposition occurred on October 20. The first set in each schedule, therefore, was made within four days of opposition; the second, within eleven days of it; the last, from fourteen to thirty-two days after it. That there is a systematic increase in the equatorial measures is apparent. That it is not paralleled by a corresponding increase in the polar ones shows instantly that it can hardly have been due to systematic error in the observer, since in that case both sets of measures should, in all probability, have been affected.

Now as all the measures had previously been corrected for refraction, irradiation, phase and tilt, the means of each diameter should have agreed with themselves. The polar did so in a very satisfactory manner; the equatorial not only did not, but they differed in proportion to their distance in time from the date of opposition. Now the only factor that increased in proportion to the distance in time from opposition was the phase. The direct effect in the way of decreasing the equatorial diameter had already, as we have seen, been allowed for; what is more, it is a correction susceptible of great accuracy, since it depends upon the motions and relative distances of the earth and Mars, quantities very accurately known. Besides these quantities, there is nothing which enters into the calculation but the position of the pole of Mars, and this would have to be, not only some 35 Martian degrees in error to explain the discrepancy, but would have had to have shifted obligingly to an opposite error during July and August to account for the measures taken then, as we shall see later. In other words, no such discrepancy exists.

In the case of a bare globe this direct effect would be the only effect phase could have upon the equatorial diameter; not so, however, in the case of a body not bare. If a planet possessed an atmosphere, that atmosphere would cause the phenomenon of twilight, and to an observer at a distance the effect of the twilight would be to prolong the terminator beyond what would otherwise be its limits. There would thus result a seeming increase in the equatorial diameter as the disc passed from the full to the gibbous phase. Now this increase is precisely the increase that the measures disclose.

It is furthermore worth noting that in the absence of an atmosphere, the measures of the equatorial diameter as the phase increased would not only have shown no increase, but would actually have shown a decrease, inasmuch as it would be impossible for an observer to see quite out to the edge under the diminishing illumination.

To determine the extent of the twilight thus disclosed by the measures, the angle between the radius to the sunset point and the radius prolonged to the point of the atmosphere last illuminated, had to be found. This enabled an equation to be developed, which gave for the visible twilight fringe an arc of 5°, the double of which, or 10°, is the angle which determines the duration of the twilight, or the twilight arc. On the earth this angle is 18°.

Applying the correction due to the twilight fringe, to the means previously obtained, we find the following close agreement between them:—

Polar Diameter.

October 15 to 22 inc.	9″·356
October 12 to 30 ,, ...	9″·354
November 2 to 21 ,, ...	9″·353

Equatorial Diameter.

October 15 to 23 inc.	9″·404
October 12 to 30 ,, ...	9″·396
November 2 to 21 ,, ...	9″·402

The value for the twilight band, deduced from these observations, does not measure the full breadth of that band. It gives rather a minimal value for it. For although Mars shows us a disc which is always more than half full, in which aspect an illuminated fringe of atmosphere would be more perceptible to an observer placed without than to one placed within it, provided both were at the same distance off, in the case before us the outsider is a great deal farther off. In consequence, what would be quite recognisable to one standing upon the planet's surface would be too faint to be seen by him at a distance of forty millions of miles away. The detection, therefore, of any twilight on Mars hints that the extent of that twilight is greater than appears; how much greater, we cannot at present say. A second possible cause affecting the extent of the twilight is the constitution of the Martian atmosphere. That atmosphere is practically cloudless; if, also, it be clearer than our own, the twilight would be relatively less for equal amounts of atmosphere, for the amount of twilight is, among other things, a question of the clearness of the air. In a perfectly transparent atmosphere there would be much less twilight than in one charged with solid or liquid particles.

It is to be noted that the evidence of a twilight is independent of any possible change in the value of the corrections. The only corrections that admit of uncertainty are those for the irradiation; and on examining them it will be seen that by no possible alteration can they be made equal to account for the observed increase in the equatorial diameter. Whatever alteration in them be assumed only affects somewhat the extent of the increase; it never does away with it. In other words, whatever these corrections, the fact of a twilight remains.

For the determination of the polar flattening, the measures of October 15 to 23 promise the best result, as the measures of the polar diameter on the 24th were so small, compared with those of the equatorial diameter, as to suggest error. Comparing, therefore, the polar and equatorial means of October 15 to 23, we get for the polar flattening 1/196. This, however, is probably too small; for though the polar cap was nearly non-existent during these observations, there were, on occasions, signs of its temporary reappearance, and an almost continuous brightness of the limb where it had previously existed. This by irradiation would increase the apparent polar diameter, and so decrease the resulting value for the polar flattening. If we compare each polar determination with its corresponding equatorial one, deduce the resulting polar flattening, and then take the mean of them all, we have for the polar flattening the value 1/191.

This is probably not far from the truth, although also probably a little too small, as the polar diameter was unquestionably still slightly increased beyond its real extent, by irradiation from the remains or consequences (vapour in the air, &c.) of the polar cap.

This value, 1/191, is also happily accordant with what theory would lead us to expect. Tisserand has found that with the known rotation of Mars and supposing homogeneity, the planet's flattening should be 1/175 of the equatorial diameter, while if the strata varied in density, after the manner of those of the earth, the polar flattening should be 1/227 of it. Now, assuming Mars to have been developed in general accordance with the nebular hypothesis, his strata would be neither homogeneous, on the one hand, nor, on the other, would they vary in density from the surface to the centre so markedly as is the case with those of the earth. For Mars being a smaller body, the pressure due to gravity would be less, somewhere between that of the earth and that of homogeneity, which is nothing, and the polar flattening should be somewhere between 1/227 and 1/175 of the equatorial diameter. 1/191 is, therefore, not far from the value probable *a priori*. It is interesting to have this result agree thus closely with theory, as it furnishes so much more reason for believing in the general evolution of our solar system.

Any value much less than 1/191 would require that Mars should have had at some time a much swifter axial rotation than he has now, which there is not only no ground for thinking, but much reason for thinking could not have been the case. For Mars lacks the tools for tidal friction, possessing insufficient satellites on the one hand and insufficient oceans on the other, so that even solar tides would be out of the question. Even had he possessed both requisites, it is more than doubtful if their slow action would have materially affected his form. For on the earth, which did possess them, we see that they were practically impotent to alter her shape. Any great change in Mars' period of rotation since he cooled must be looked upon, therefore, as unlikely.

For the final values of the diameters we have, allowing for a slight irradiation from the remains of the polar cap:—

True equatorial diameter	$9''\cdot40$	$''\cdot007$
True polar diameter	$9''\cdot35$	$''\cdot007$

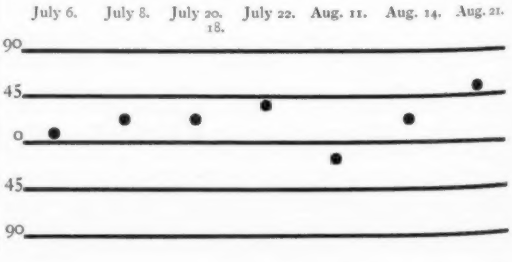
It will be noticed how near these values are to that found by Hartwig from his general discussion several years ago.

We will now consider the September observations and the first of the October ones, those taken upon the fifth of the month. The first thing we notice about them is the abnormal size of the polar measures, so large as to suggest error. On examination, however, we find that instead of mistake they give us our first recognition of the cause that has so long masked the effect of the twilight fringe. The equatorial measures, it will be seen, come out in fairly good accordance with the October and November determinations, being greater than those taken near opposition, although somewhat smaller than the November ones, the discrepancies falling probably within the errors of observation. The polar measure of October 5 is also much what we should expect, but the polar measures of September 20 and 23 are apparently unaccountably larger. If we consider, however, the dates at which they were taken, we shall at once perceive a cause capable of producing the apparent increase. For in September and early in October the polar cap was still in existence. Now the south polar cap is eccentric to the pole, being situated some 5° from it, and from Mr. Douglass's micrometric measures of its position in October, in longitude 59°. As during the observations the south pole was tipped towards the observer, the polar cap was carried, in consequence of the planet's rotation, now in upon the disc, now out upon the limb. Now, if it chanced to be upon the limb at the hour at which the measures were made, its excessive irradiation would produce just such apparent increase in the polar diameter as was observed. On calculating its position for the hours of observation on September 20 and 23, it appears that at those times it was in fact upon the side of the pole toward the limb. Here, then, we have the *deus ex machina* in the matter. To clinch the conclusion, we find on calculating its position for the observation on October 5, when it suddenly measured small again, that at that hour the polar cap was upon the hither side of the pole. Such was also the case on October 12. The discrepancy thus stands accounted for. On October 13, very obligingly, the polar cap practically vanished just in time not to interfere with the most valuable measures at and near opposition.

That such is the explanation of the change in the polar diameter, comes out still more markedly from the July and August measures. Turning to those measures we find that the position of the polar cap is an all-important factor in them. Indeed, it is possible to follow its change of place upon the disc from its effect as reflected in the measures. This will appear at a glance from the accompanying diagram of the July and August measures of Mr. Douglass. A similar sequence of position and effect is apparent in Prof. Pickering's measures made at the same time.

COMPARISON OF POSITION OF POLAR CAP AND MEASURE OF POLAR DIAMETER.

The distance of the point from the medial line shows the angular position of the polar cap from the pole at the times of observation; 90° denoting its lower, and 90° its upper meridian transit. At its lower culmination it was at its nearest to the centre of the disc; at its upper, nearest the limb. The measures show the corresponding effect in irradiation.



			Polar.			
9″·85	10″·29	9″·57	9″.46	9″·41	9″·40	9″·34
			Equatorial.			
9″·67	10″·08	9″·48	9″·33	10″·03	9″·75	9″·41
In relative values						
			Polar.			
1019	1021	1009	1014	938	965	993
			Equatorial.			
1000	1000	1000	1000	1000	1000	1000

At first sight it would seem that the later August measures do not support the rule. Closer consideration will, however, show that they do. For while in July the polar cap was still large, and in consequence reached to the limb, even when its centre was at some distance from it, by August it had dwindled to so small a patch as to be incapable of doing so when at the same angular distance away. Taking account of this fact, it will be seen that the effect is quite in accordance with the position, as comes out clearly in the relative values for the two diameters of August 14 and August 21.

It will now be evident why so large, and intrinsically so unmistakable, an effect as that of the Martian twilight should hitherto have escaped detection; the reason being that the twilight effect and the irradiation from the polar cap each increased their respective diameters to a simultaneous augmentation of both, conspiring each thus to mask the other.

Had measures been continued through a series of months, and been made in sufficient number, both causes must have made themselves evident. For both are periodic, and their periods are not the same. The irradiation from the polar cap has a primary period of thirty-seven days, a secondary one of a Martian year as well as a third depending on the tilt of the pole toward the earth; that of the twilight fringe a varying one of about thirteen months. But as previous measures have been made quite regardless of the twilight effect, and largely regardless of the polar cap, regardless, that is, of its varying position, the results have merely disagreed with each other, and the disagreements been credited to errors of observation. One result of this was discordance in the value of the polar flattening.

When we take both causes into account we find that the means of the July and August observations confirm the October and November ones.

For by comparing the values of the polar diameter when on and away from the limb, it is possible to deduce both the amount of the irradiation from the polar cap and the value of the twilight band from the measures themselves. The results in the case of Mr. Douglass agree with those of his October-November measures. In the case of Prof. Pickering, there is the same relative difference between the determinations, although the absolute values are all smaller.

That in the table the corrections to the July and August measures differ from those applied to the later ones, comes from the different manner of their taking; in the July and August measures the longitudinal thread of the micrometer having been set to the phase axis or perpendicular to it, instead of to the polar one.

In Mr. Douglass' determinations the value for the twilight arc comes out 8°. This is somewhat smaller than the result from the November measures. But a smaller value is precisely what should have been found. For the greater the phase angle, the less the foreshortening, which foreshortening by massing the illumination lets the fringe of light become evident farther out. Now the average phase angle was 43° in July and August, as against $18\frac{1}{2}$° in November.

From Prof. Pickering's measures the twilight arc comes out greater, or 11°, and by inference would have come out greater still in November.

Thus it appears that measures made by separate observers, and measures made before and after opposition, all confirm each other to the existence of a twilight band upon the planet.

PERCIVAL LOWELL.

THE FOUNDATIONS OF ENGINEERING EDUCATION.[1]

LET us consider what is the education which a young man needs to fit him for the profession of engineering, whatever be the special line of engineering which he proposes to follow. And, before discussing the details of what he ought to study, let us consider what it is that we desire to accomplish by giving him an engineering education. Naturally, we wish, as far as any education can accomplish it, to put him in the best condition to meet and grapple with the duties, the problems, and the responsibilities of his profession, as they arise.

There are two things which are absolutely necessary to make a successful engineer: first, a knowledge of scientific principles and of the experience of the past; and second, his own experience. The last cannot be given in a school, and each one must gain it for himself in his practice.

But the greater his familiarity with scientific principles and the experience of the past, the more able will he be to advance in his profession, and to be trusted to assume responsibility; indeed, if a man is ignorant of certain details and knows he is ignorant, he can—and if he is the right kind of a man, he will—take pains to learn them, if they bear on the work he has in hand; but if he is ignorant of scientific principles, it is very likely that he does not know he is ignorant, or, if by good luck he becomes aware of the fact, it is next to impossible for him to devote the time and study necessary to correct his ignorance while his mind is busy with his daily work.

Moreover, a man who is not familiar with the scientific principles which concern his work is not a safe man to trust with responsibility; for scientific principles are merely the laws of nature, as far as known, as shown by the experience of the past.

Hence it is that the first and most important thing to be done for the student is to give him a thorough drill in the scientific principles which find their application in his profession. It is in the school that this knowledge may best be acquired, since it is only with great difficulty that principles can be mastered after the student begins practice, and then as a rule but very imperfectly; and this view is borne out by those engineers who have been successful, and who have had to acquire their knowledge of scientific principles little by little, and as best they could, during the practice of their profession. Too much cannot be said by way of insisting that a thorough mastery of such scientific principles far outweighs in importance anything else that can be done for the student; and this is so true, that it is a decided mistake to neglect it in order to impart to him greater skill in such processes as will probably engage his attention the first year after he goes to work, as, for instance, to make him a skilful surveyor, a finished machinist, or an elegant draughtsman. Greater skill can far more easily be acquired after he goes to work than can scientific principles, and if this mistake is made the consequences will probably pursue him throughout his professional life.

The two fundamental sciences upon which the scientific principles of engineering are especially dependent are mathematics and physics, and no proper course in engineering can be arranged without insisting upon these fundamentals.

Let us begin with the subject of pure mathematics, and consider what portions should be studied, how they should be studied, or rather how they should be known, and of what service they are to the engineer after they have been mastered; bearing in mind that, in accordance with the opinions already expressed, the course of study should be laid out with direct reference to the needs of the engineer; and that when it is so laid out, it will, by the very fact that it leads to a definite end, subserve best the purpose of true education, and hence of developing the powers of the mind. Probably the best definition of mathematics is that given by Prof. Benjamin Pierce, who defined it as "the science of drawing necessary conclusions." This definition, of course, includes formal logic, and hence embraces more than is ordinarily understood by mathematics. We may assert, however, that the only function of mathematics is to draw necessary conclusions from the assumed data. Mathematics has nothing whatever to do with the correctness or incorrectness of the data. If these are correct, the conclusions deduced by mathematics will also be correct; whereas, if the data are false, the conclusions deduced by mathematics will be false.

Thus, if we require the sum o. a certain set of numbers, the process of addition will give the correct result, provided the numbers added are the right ones; but if the numbers added are not the right ones, the result of the addition will not be the one desired. Indeed, we might compare pure mathematics to a mill—it will only produce good meal when the corn furnished to it to grind is of good quality; and if the corn is poor, the meal pro-

[1] Extracted from a course of lectures delivered in the Lowell Institute, Boston, by Prof. G. Lanza, Professor of Theoretical and Applied Mechanics, Massachusetts Institute of Technology, and published in the *Journal* of the Franklin Institute.

duced will be poor. With the selection of the corn which it is to grind, the mill has nothing to do.

No natural law can be discovered or proved by mathematics alone; the discovery or proof of natural law requires experiment and observation in all cases.

Just as arithmetic is a means of making calculations of certain kinds, so there are other kinds of calculations that can only be performed by the use of mathematics higher than arithmetic; some kinds require algebra, some geometry, some trigonometry, some descriptive geometry, some analytic geometry, and some the differential and integral calculus; while others yet require higher mathematics. Now, inasmuch as every one can easily understand the necessity of arithmetic for the purpose of making the calculations, and drawing the conclusions which come within its province; so, it follows that the engineer should have a thorough working knowledge of whatever portions of pure mathematics he needs, to make the calculations that are liable to arise in his work, and also to draw the necessary conclusions which concern the engineering and scientific subjects with which he must deal in his profession. This latter is an all-important matter; for, if our prospective engineer is to be fit to assume responsibility at some portion of his career, before he allows himself to use a formula in practice, he ought to know just how it is deduced, and what are the assumptions that were made in deducing it.

The rule-of-thumb engineer ignores this matter, and allows himself to risk the money, the safety, and the lives of his fellow men by making use of constants and mathematical formulæ found in some hand-book or elsewhere; using these constants and formulæ blindly, without knowing how they were deduced, or whether they have any reasonable foundation to stand on; or, in other cases, contents himself with merely guessing at what should be the dimensions of the various parts of a structure or machine. The natural result of such a course is poor work, and often disaster; and the world is rapidly waking up to this fact, so that important engineering work is being less and less entrusted to these rule-of-thumb engineers.

Now, I may say that knowledge of at least all these subjects mentioned in my communication—through the differential and integral calculus—is necessary for our prospective engineer.

As to descriptive geometry, that is classed by many, not as mathematics, but as a branch of drawing. It is the mathematical work upon which the making of engineering drawings of all sorts is based, and hence I have put it in this list.

So general has the conviction become that the engineer needs some knowledge of the differential and integral calculus, that it is not necessary for me to cite cases where he must use it if he is to perform his work intelligently and not by rule-of-thumb. Differential equations is a subject which is sometimes classed with the differential and integral calculus, and sometimes as a separate subject. It is one that should, if possible, be learned at least to a small extent, though the more that is known about it the better.

As to the special work to be done in each of these subjects, it is a matter of judgment with the one who lays out the course, and I shall not weary you with these details; but I must explain what ought to be the result aimed at, in other words, how the student should know his mathematics.

I might express my idea by saying that he should acquire the ability to use it as a tool, but, when I say that, I mean not merely as a tool for making computations, but also as a tool for drawing necessary conclusions of the kinds that apply to his engineering work; and this last is the feature which is most frequently lacking in the mathematical instruction given to engineering students.

By one method often pursued in teaching mathematics, the student is made to grind through a certain round of operations over and over until he has been so drilled in performing them mechanically that he can *perform* a similar problem. By this method, he is only taught to use it as a tool for making computations.

Another method, often pursued, is to exercise the student's ingenuity in performing a variety of (sometimes puzzling) problems which are of purely abstract interest, and are not planned in such a way as to bear upon the class of problems liable to arise in engineering work or study. This course probably tends to make the student do more thinking, but does not direct his thinking in the channel most useful for the prospective engineer. To accomplish the desired object in the teaching of mathematics, it is, of course, necessary that the teacher should be able to grasp the requirements of the engineering courses, and should know the special kind of use that the prospective engineer will have for his mathematics in later life.

Another important matter, the accomplishment of which concerns the treatment of the subjects of a mathematical nature that follow in his course, rather than the treatment of the pure mathematics itself (though the mathematical department can help in this matter), is that the student should be taught to distinguish between the mathematics of the work, and the assumptions made at the beginning, or in the course of it, respecting the proposition he is dealing with.

Perhaps I might sum up a part of the foregoing by saying that the student should be taught to think, and that the attempt to teach him to think should begin as early as possible in his course, and be kept up throughout. It is much easier for the average student to learn a lesson to recite by rote even if it contains a lot of formulæ, than it is to do a little solid thinking himself, and yet the more we can make him think the more successful in every way will he be.

Perhaps those of you (if there be any) who teach mathematics may think that the standard I have set is high. I admit that it is, and also that it requires hard work, good judgment, and the qualities of a good and efficient teacher, not only in laying out the course, but even more in teaching the class. Nevertheless, this standard is the one that is needed, and good judgment, and good teaching can at least approach near to it within the time that can be afforded in our engineering courses, even with such previous mathematical preparations as can be obtained to-day by the students before they enter the engineering schools; and as fast as it becomes possible to raise the standards of admission, the standard I have set can be even more fully realised.

The other fundamental science which I have mentioned is physics. It may be defined as that department of natural science which treats of the laws governing the various manifestations of energy (as gravitation, sound, heat, light, electricity, &c.).

It deals with the natural law as it applies to just those classes of bodies, and substances with which the engineer does his work. Indeed, physics is a very general term, and might be made to include a great many subjects that are usually called by some more special name. For instance, mechanics is sometimes spoken of as a separate science, and sometimes as forming a part of physics, and, moreover, under any definition physics includes a part of mechanics.

Practically, a course in physics is the suitable preparation for a proper understanding of the scientific principles of most of the engineering work with which the student will come in contact. Treating, as it does, of the laws of nature, the more thoroughly an engineer knows it, the more successful will he be, and an ignorance of these laws can only result in failure.

Mechanics, light, sound, heat, and electricity, are all matters that concern the profession of the engineer so intimately that he cannot afford to neglect a careful study of their first principles. It is unnecessary for me to say, therefore, that there is no portion of the work usually treated in the best and most thorough courses of general physics but what should be included in the course of our prospective engineer.

Then, a certain amount of work in the physical laboratory is of great importance for the student, for it teaches him how to ask questions of nature, and how to get correct answers; in other words, how to make careful and accurate experiments, and this is a matter that intimately concerns the engineer. It is true that the greater part of his experimental work will have to be performed on a considerably larger scale than that usually carried on in a physical laboratory; but, on the other hand, some of his most important and delicate work involves the doing of just such experimental work as he is taught to carry on in a well-organised and well-equipped physical laboratory; and also the performance of these physical laboratory experiments is a proper introduction to his later course of experiments on the large scale, drilling him in accuracy and care while working on small amounts of material.

Indeed, I might mention quite a number of experiments which are all-important to the engineer, and in regard to which, it would be difficult to decide whether they should be called physical laboratory or engineering laboratory experiments, since they often have to be performed in both. Thus, the calibration of thermometers is a matter that is properly taught in the former, and yet the engineer who is to do delicate engineering work is liable to have to calibrate his thermometers, or at least to make a careful and accurate comparison with a standard which he or

some one else has calibrated. Again, the determination of the mechanical equivalent of heat is a matter of vital importance to the engineer, but the best and most accurate work thus far upon the subject has been done by Prof. Rowland, a physicist, in his physical laboratory.

As a rule, when experiments are to be performed on the large scale they get beyond the possibilities of a physical laboratory. In this category we may place such experimental work as the testing of steam engines and steam boilers, the testing of the strength of materials of construction on a practical scale, &c.; but, in order to carry out these tests with proper accuracy, we have generally to perform delicate measurements, as, for instance, measurements of temperatures, &c., in the first, and measurements of very small elongations or shortenings in the second case, and consequently have to use the suitable apparatus with the necessary degree of accuracy.

Since we have just been considering mathematics and physics, which may be called general sciences, perhaps a few words should be said in regard to chemistry. I cannot claim for it a similar position of fundamental importance in the engineering part of an engineering course that belongs to mathematics and physics. Nevertheless, a certain amount of chemical knowledge is of great importance to all engineers; but when they have passed this point, although a farther knowledge would be useful, it is not one of the most important things. The chemical composition of fuels, of steels and irons, of cements, of oils, and of other materials, is a matter that directly concerns the engineer. It is true that he can usually have his chemical analyses made for him, and generally would better do so; but he must know enough of chemistry to understand the bearing which the chemical composition of his materials have on their use in engineering work. Some knowledge of industrial chemistry is also desirable, so that he shall understand the nature of the processes performed in manufactories in which chemical processes on a large scale are performed.

The instruction in chemistry should, if possible, be given very early in the student's course. In the case of the Massachusetts Institute of Technology, and also, I think, in that of several other schools, both lectures and laboratory work in chemistry are given in the first year, and when this is done the instruction in chemistry fulfils another important function, viz. it introduces the student at the very threshold of his course to a species of scientific work that obliges him to think, and this, in a direction in which, as a rule, he has not been trained in the preparatory schools. Especially is this true of the laboratory work, for, by observing the results of experiments which he himself makes, he must learn how to interpret the replies of nature; and as chemistry, unlike mathematics, is an experimental science, it trains the thinking powers of the student even more than do his algebra, geometry, and trigonometry.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

A PROSPECTUS of the course in practical chemistry at the Polytechnic Institute of Brooklyn has been received, and it indicates that very efficient work is carried on at the Polytechnic. The course, which is under the direction of Prof. P. T. Austen, appears to be adapted in every way to meet the wants of the day, and to train competent analytical and technical chemists. The claims of pure chemistry are also recognised, facilities being given for post-graduate work in it, as well as in applied chemistry and chemical engineering.

THE Department of Science and Art has issued the following lists of Scholarships and Exhibitions just awarded:—Whitworth Scholarships (tenable for three years), £125 a year each: Arthur H. Barker (24), engineer; George W. Shearer (21), apprentice engineer; Percy Nicholls (24), engineer; Harold R. Cullen (21), engineer. Whitworth Exhibitions (tenable for one year), £50 a year: Charles E. Goodyear (21), shipwright; George M. Brown (23), draughtsman; Norton Baron (22), engineering student; Harry Jackson (20), engineering student: Edward M. Leflufy (22), engine-fitter apprentice; Arthur E. Hyne (21), fitter apprentice; Robert McMillan (20), engineer apprentice; John W. Roebuck (23), fitter; George Follows (24), engineer; Arthur J. Baker (19), engine-fitter apprentice; William D. Ross (21), fitter; Frank H. Phillips (20), engineer apprentice; Henry T. Hildage (20), fitter; William P. Jones (25), marine engineer; John W. Milner (20), mechanical engineer; William Bayliss (20), apprentice fitter; John B. Shaw (21), engineer; James Walker (22), engineer; William H. C. Kemp (21), engineer apprentice; William J. Talbot (23), engineer; Henry C. Trigg (24), draughtsman; Duncan R. McLachlan (24), engineer; George A. Robertson (21), engineering student; Charles H. Imrie (22), engineer; William McG. Wallace (20), apprentice fitter; William J. Gow (20), apprentice fitter; William Lauder (20), draughtsman; Samuel A. Clarke (25), draughtsman; Edmund B. Ball (21), engineer student; Jabez W. Ashdown (20), engineer apprentice.

THE list of successful candidates for Royal Exhibitions, National Scholarships, and Free Studentships (Science) is as follows:—National Scholarships for Mechanics: Edmund R. Verity (19), student; George Patchin (17), engineering student; Harry Jackson (20), engineering student; William Ditchburn, jun. (19), teacher. National Scholarships for Chemistry and Physics: Thomas S. Price (19), student; Franz E. Studt (21), tailor; Herbert Bailey (18), student; William Bennett (16), student; John W. Barker (18), laboratory assistant. National Scholarships for Biological Subjects: Thomas G. Hill (19), student; Ernest A. Scott (17), student. National Scholarships: Charles E. Goodyear (21), shipwright; Edward M. Leflufy (22), engine-fitter apprentice; William H. James (22), student; William T. Clough (18), student; Herbert Halliday (22), student; William Cameron (18), laboratory assistant; Ernest Hibbert (15), student; Sidney E. Lamb (21), engine-fitter apprentice; Joseph Lister (19), teacher; William Parker (19), student; Ernest T. Harrison (18), laboratory assistant. Royal Exhibitions: George E. Clarke (17), student; Edward C. Hugon (16), student; Thomas G. Procter (19), engine-fitter apprentice; John A. Tomkins (20), scientific instrument maker; William T. Swinger (20), engineer; John W. Roebuck (23), fitter; Robert L. Wills (21), shipwright apprentice. Free Studentships: William D. Ross (21), fitter; Leonard W. Cox (21), student; Edgar R. Sutcliffe (20), engineer; William P. Jones (25), marine engineer; Percy M. Hampshire (19), lecture assistant; William J. Talbot (23), engineer.

SCIENTIFIC SERIALS.

Bulletin de l'Académie des Sciences de St. Pétersbourg, 5th series, t. ii. No. 4, April 1895.—Proceedings, in which we notice the discovery, by G. Schneider, in Prof. Kovalevsky's laboratory, of lymphatic glands in the earth-worm, *Dendrolæna rubida* (Crimea), and in *Perichæta;* as well as a communication by E. Burinsky, on his method of restoring by means of photography the writing in old documents which time has rendered invisible. A number of good negatives having been taken on collodion pellicules, they are superposed, and the visibility of the faintest markings is rendered still greater by means of a "contrast positive" obtained with regulated artificial light.—Definitive researches into the variations of latitude at Pulkova, on the ground of older observations made with the great vertical circle, by A. Ivanoff (in French). The previous memoirs of the author on the same subject being considered as first approximations only, the definitive formulæ are now given. The observations of the years 1863–1875 and 1842–1849 are treated for that purpose separately. Both series lead to formulæ which agree very well with the formula given by Mr. Chandler in the *Astronomical Journal*, No. 322; however, the Pulkova observations of the first-named period seem to point to the necessity of slightly reducing the half-amplitude of the yearly term in Chandler's formula. Two long series of Pulkova observations thus fully confirm Mr. Chandler's conclusions.—On the measurements and calculations of some photographic charts of the stars, by F. Renz (in German). A catalogue of all stars, down to the magnitude 11·0, which were occultated by the moon during the last eclipse, was given in the *Astronomische Nachrichten*. It appeared, however, that occultations of stars down to the twelfth magnitude could be observed at several observatories. Accordingly, the corresponding region of the sky was photographed by Prof. Donner with such an exposure (25 minutes) as to obtain the stars of twelfth magnitude as well, and F. Renz measured their positions with the Pulkova Repsold apparatus. The Potsdam photographs of the same region, made in 1891, were also re-measured, while the right ascensions of thirty-five fundamental stars were accurately determined at Pulkova with the meridian circle. The agreement between the different plates is quite satisfactory; and no distortion of the field could be detected. However, there are certain small systematic errors which cannot yet be well

explained. Thus, the right ascensions on plate i. are on the average by 0·047s. greater than the values deduced from plate ii.—The Arachnides collected by G. Potanin in Mongolia in 1876–1879, by E. Simon (in Latin). Part i. Aranæ and Opiliones; forty-one species are mentioned and described, nineteen being new species.—Do the spurs of the Carpathians penetrate into European Russia? by General A. Tillo (in Russian). The question is answered in the negative. Supan and Lehman, in Kirchhoff's "Länderkunde von Europa," trace the limits of the Carpathians outside the boundaries of Russia; so also the Russian geologists, Barbot-de-Marny and Karpinskiy, did not see continuations of these mountains either in Poland or in Russia. The new hypsometrical map, now compiled by the author on a larger scale (27 miles to the inch), confirms this view.—New or little known Ixodidæ in the museum of the St. Petersburg Academy, by A. Birula (in Latin). Eight new species are described and figured on two plates.

Memoirs (*Trudy*) *of the Kharkoff Society of Naturalists*, vol. xxvii., 1892–93.—Obituary of I. Th. Levakovsky, by A. Guroff, with a portrait.—Researches into the crystals of kermesite and uranotil, by P. P. Piatnitzky.—The Alguæ of the bays and peat-bogs of the Dnieper, in the government of Poltava, by M. Alexenko. This flora is poor, the *Cladophora*, *Confervæ*, *Enteromorpha*, and *Ulotrix* prevail, while Desmidiaceæ and Protococcoideæ are very rare; 371 species are mentioned.—The flora of the Central Caucasus, by I. Akinfieff, part i. (see Notes, vol. lii. p. 304).—On the part played by hydrocarbons in the inter-molecular respiration of higher plants, by W. Palladin. It had been shown by Diakonoff (*Ber. d. deut. bot. Ges.*, 1866) that certain fungi give up carbonic dioxide during their inter-molecular breathing, only when the surrounding feeding medium contains a substance capable of fermenting. It was desirable to verify whether the same is true with higher plants, but the difficulty was in the fact that the cellular sap always contains glucose, which itself is capable of fermenting. By a series of experiments on etiolated leaves, the author now confirms Diakonoff's conclusions for higher plants as well.—Short preliminary notes in the Addenda. Vol. xxviii., 1893–1894.—Geological description of Kharkoff town, with map and profiles, by P. Poustovitov.—On the part played by the secondary parallel chains in the grouping of forests and steppes in West Caucasus, by A. Krasnoff. An answer to G. Akinfieff's criticisms.—Materials for the Algæ flora of the government of Kharkoff, by M. Alexenko; 407 species are described.—Preliminary report on a geological excursion in the government of Kherson, by P. Piatnitzky.—Biological observations, by W. Taliev. A series of various observations of facts relative to the life of plants, which have hitherto attracted but little or no attention, chiefly relative to fertilisation, colouration, movements of plants, and heliotropism in connection with the affluence of sap.—On the flora of the basin of the Chakva, by A. Krasnoff, being a preliminary report of a botanic excursion into the province of Batum, containing an excellent general description of the vegetation, poor in species, but attaining a luxurious development of the individuals.—On the lichens of the neighbourhoods of Kharkov, by W. Tschernov; fifty-five species are described.—Chemical studies on the seeds of *Myristica fragrans*, by W. Palladin, being a note on a special substance which is found in several seeds, but neither in the leaves or in the twigs, and which is now studied in Prof. Schultze's laboratory at Zurich.—Preliminary report on botanical researches in the Verkhnednieprovsk district of Ekaterinoslav, by I. Akinfieff; twenty-six species, new for South Russia, have been discovered.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 12.—M. Marey in the chair.—Observations of planets made at Marseilles Observatory, by M. Coggia. The observations were made with the 0·26 m. equatorial, and for the planets BZ and CA (Charlois).—On algebraical surfaces which admit a continuous group of birational transformations, by M. Paul Painlevé.—On a special microscope for the observation of opaque bodies, by M. Ch. Fremont. The novelty in the microscope described, consists essentially in the method used for obtaining vertical illumination of the object, applicable with high powers. A concave mirror is arranged obliquely *inside* the microscope tube to reflect downwards a beam of light entering at a side aperture in the tube. The light passes through a prism which reduces the rays to parallelism with the axis of the microscope and then through the lenses of the objective to the object. The concave mirror and the prism are pierced centrally by a conical tube along which travel the rays of light from the object, the image being formed and magnified by the eye-piece in the usual way. M. Marey remarked on the great use the new modification would have in the chronophotographic study of the movement of microscopic beings.—On some melting and boiling points, by M. H. Le Chatelier. From the experiments made, it is probable that the melting point of gold determined by M. Violle to be 1045°, is a little low. The error is certainly not more than 20°, and the results so far obtained would not justify the alteration of the pyrometer scales in actual use.—On certain potassium derivatives of quinone and hydroquinone, by M. Ch. Astre. A number of potassium derivatives are described, concerning which it is stated: the action of metals on quinone, together with the existence of oxy-potassium compounds yielded by quinone and hydroquinone (to be described in a coming paper) confirm the diketonic nature of quinone. The formation of these compounds and the passage of some of them from the hydroquinone to the quinone series, allow a formula to be given to quinone clearly expressing its diketonic character and accounting for its numerous reactions.—A theorem concerning the separation of the roots of numerical equations of every degree, by M. Teguor.—A white rainbow, by M. E. Kern. A lunar rainbow observed at 10 p.m. August 5.

BOOKS, PAMPHLET, and SERIALS RECEIVED.

BOOKS.—British Birds: W. H. Hudson (Longmans).—Lectures on Elementary Navigation: Rev. J. B. Harbord (Potter).—Polyphase Electric Currents and Alternate-Current Motors: Prof. S. P. Thompson (Spon).—Transactions of the Australasian Institute of Mining Engineers, Vol. 2 (Adelaide).

PAMPHLET.—The Recent Evolution of Surgery: A. P. Gould (K. Paul).

SERIALS.—Journal of the Chemical Society, August (Gurney).—Proceedings of the Physical Society of London, August (Taylor).—Bulletin of the American Mathematical Society, July (New York, Macmillan).—Natural History of Plants: Kerner and Oliver, Part 15 (Blackie).—Bulletin de L'Académie Royale des Sciences, &c., de Belgique, 65e Annee, No. 6 (Bruxelles).—Astrophysical Journal, August (Chicago).—Royal Natural History, Part 22 (Warne).

CONTENTS.

s of
rism
the
nag-
rked
ono-
—On
From
nt of
The
far
cales
and
deri-
ction
oxy-
e (to
re of
ge of
allcw
tonic
orem
ons of
Kern.

VED.
n Ele-
lectric
on).—
Vol. 2

Paul).
roceed-
of the
Natural
tin de
No. 6
Natural